SOLAR RESEARCH

Sherratt & Hughes
Publishers to the Victoria University of Manchester
Manchester: 34 Cross Street
London: 60 Chandos Street W.C.

Astron
I

Transactions

of the

International Union

for Co-operation in

SOLAR RESEARCH

Vol. I.

(First and Second Conferences.)

113361
31/5/11

MANCHESTER
At the University Press
1906

PREFACE.

The delay in the appearance of this volume has been due to various causes, among which the time taken up by correspondence with authors living in different countries was the most serious. Although primarily intended to give a record of the proceedings of the Oxford Conference, the volume includes also an account of the previous Conference held at St. Louis and of the preliminary transactions connected with the establishment of the Union. It is hoped that the complete record of the history of the organisation, which is here presented, may be of some value to those interested in Solar Physics.

Among the papers printed, some have already appeared in other publications, but some are new, and attention may be specially drawn to Professor Fowler's communication of his observations of the spectra of sun-spots, which has not hitherto been printed. As this work bears directly on the systematic organisation of the records of sun-spot spectra, which has been undertaken by the Union, it has been thought desirable to print it in full.

For the Executive Committee,

ARTHUR SCHUSTER.

The Central Bureau,
The Physical Laboratories,
University of Manchester,

August, 1906.

TABLE OF CONTENTS

PART I.

Origin of the Union.

PAGE

No. 1. Circular sent out by the Committee on Solar Research of the National Academy of Sciences 1

No. 2. Societies originally invited to form part of the Union 4

PART II.

Proceedings of the First Conference on Solar Research held at St. Louis, September 23rd, 1904.

No. 3. Minutes of the Meeting of Delegates to the Conference on Solar Research, held in the Hall of Congresses, St. Louis, September 23, 1904 5

PART III.

Papers submitted to the First Conference on Solar Research, held at St. Louis, September 23rd, 1904.

No. 4. Co-operation in Solar Research. By George E. Hale 11

No. 5. Remarks on Standard Wave-Lengths. By Henry Crew 19

No. 6. Rapport sur la necessité d'établir un nouveau système de Longueurs d'Onde Etalons. Par A. Pérot et Ch. Fabry 26

No. 7. New Standards of Wave-Length. By H. Kayser 37

No. 8. The revision of Rowland's system of Standard Wave-Lengths. By Lewis E. Jewell 42

PART IV.

Preparations for the Second Conference.

PAGE

Introductory Remarks 58

No. 9. Circular Letter addressed to Members of the Union 59

No. 10. Circular Letter addressed to Members of the Union 62

No. 11. Intensity of Solar Radiation. (Arthur Schuster) 63

No. 12. Report on Radiation. (M. J. Violle) 66

No. 13. Work with the Spectroheliograph. (George E. Hale) 76

No. 14. Replies to the above communications, received from Members of the Union and Others.

I. Standards of Wave-Length.

1. Professor H. Kayser 80
2. National Academy of Sciences 82
3. Professor T. Hartmann 83
4. Professor Ames... 86
5. Sir David Gill 87
6. Professor C. G. Abbot 87
7. Professor G. E. Hale 88
8. Professor Arthur Schuster 89

II. Solar Radiation.

1. Professor Abbot 90
2. Dr. Gilbert Walker 100
3. Professor Ricco... 102
4. Professor Hale 103
5. Mr. Filipe Valle 104
6. Professor E. B. Frost 105
7. Professor Arthur Schuster 105

III. Spectroheliograph.

1. Professor Hale 107
2. Professor Hale 110
3. Professor Ricco... 111
4. Mr. C. Michie Smith 112
5. Mr. Filipe Valle 114
6. Professor Abbot 116
7. Professor Frost... 117

Preparations for the Second Conference—*Continued.*

IV. Spectra of Sun-spots — Page

1. Professor Hale 118
2. Professor Fowler 119
3. Mr. C. Michie Smith 121
4. Fr. Julius C. Fényi, S.J. 122
5. Professor Ricco... 124

V. Solar Rotation.

1. Mr. Walter S. Adams 124
2. Dr. Halm 127

VI. Co-operation and General Matters.

1. Dr. Gilbert T. Walker 127
2. Professor Wolfer 127
3. Mr. David Hadden 128

No. 15. Two Letters addressed by Prof. Callendar to the Editor of the "Astrophysical Journal" 129

No. 16. Communication from Mr. W. A. Michelson ... 131

No. 17. Communication from Mr. Evershed 133

No. 18. Vorläufiges Programm der Russischen Abteilung der Internationalen Sonnencommission ... 135

No. 19. Communication from the Imperial Academy of Science in Vienna as Leading Academy of the International Association of Academies 139

No. 20. Suggestions regarding the constitution of International Union for co-operation in Solar Research 141

Part V.

Second Conference of the International Union for Co-operation in Solar Research, held at Oxford. September, 1905.

No. 21. Minutes of Proceedings 147

PART VI.

Papers Communicated to the Second Conference, held at Oxford, September, 1905.

PAGE

No. 22. The Compensating Pyrheliometer. By Knut Ångström 178

No. 23. On the methods for the investigation of the Changes in Solar Radiation. By Knut Ångström 180

No. 24. Remarques sur l'état actuel des Recherches Solaires et sur les moyens de les améliorer. Par M. H. Deslandres 184

No. 25. Sur le sens précis de mots anciens et sur le choix de mots nouveaux. Note de M. H. Deslandres 193

No. 26. Proposal for an investigation of the local inequalities of the Sun's radiation field. By W. H. Julius 196

No. 27. Sur le mouvement de la matière à la surface solaire. Par Prof. A. Bélopolsky 198

PART VII.

Paper Accepted by the Executive Committee for Publication in the Transactions.

No. 28. Observations of the spectra of the Sun-spots, region b to E. By A. Fowler 201

PART VIII.

Resolutions Passed at the Oxford Conference and Text of the Constitution as finally Adopted.

No. 29a. Resolutions concerning Standards of Wave-Length 230

No. 30a. Resolutions concerning the measurement of Solar Radiation 231

No. 31a. Resolutions concerning co-operation in the investigation of the Sun's surface 232

Resolutions Passed at the Oxford Conference and Text of the Constitution as finally Adopted—*Continued.*

		Page
No. 32a.	Resolutions concerning the relations of the Union to the International Association of Academies	233
No. 33a.	Resolution concerning the desirability of forming additional Committees for co-operation with the Union	233
No. 34a.	Resolution concerning the interpretation of Resolutions printed in different languages	234
No. 29b.	Résolutions concernant les Etalons de Longueurs d'Onde	234
No. 30b.	Résolutions concernant la mesure de la Radiation Solaire	235
No. 31b.	Résolutions concernant les recherches sur les phenomènes de la surface du Soleil	236
No. 32b.	Résolutions concernant les relations de l'Union avec l'Association Internationale des Académies	237
No. 33b.	Résolution concernant la desirabilité de former des Commissions Spéciales dans les pays qui n'ont pas de Sociétés s'occupant de Recherches Solaires	237
No. 34b.	Résolution concernant l'interpretation des Résolutions imprimées dans plusieurs langues	237
No. 29c.	Beschlüsse über die Feststellung der Wellenlängen Normalen	238
No. 30c.	Beschlüsse welche die Messung der Sonnenstrahlung betreffen	238
No. 31c.	Beschlüsse welche sich auf die Untersuchungen der Erscheinungen auf der Sonnenoberfläche beziehen	240
No. 32c.	Beschlüsse über die Beziehungen der Vereinigung zur Internationalen Association der Akademieen	241

Resolutions Passed at the Oxford Conference and Text of the Constitution as finally Adopted—*Continued.*

		Page
No. 33c.	Beschluss über die Zweckmässigkeit weiterer Ausschüsse zur Mitwirkung mit der Vereinigung	241
No. 34c.	Beschluss über die Ausdeutung der in verschiedenen Sprachen abgefassten Beschlüsse ...	242
No. 35a.	Constitution of the International Union for co-operation in Solar Research	242
No. 35b.	Constitution de l'Union Internationale pour la coopération dans les Recherches sur le Soleil	247
No. 35c.	Constitution der internationalen Vereinigung für gemeinsame Sonnenforschung	252

Solar Research.

PART I.

Origin of the Union.

The International Union for Co-operation in Solar Research took its origin in a circular letter addressed by Professor George E. Hale to a number of men of science interested in the subject. Favourable answers having been received, Professor Hale approached various Societies and Academies requesting them to appoint committees for the purpose of initiating and organising co-operation. An invitation to attend a meeting to be held in connexion with the International Congress of Science at the St. Louis Exhibition was issued at the same time. The following is the text of Professor Hale's second letter giving the history of the proceedings which led to the formation of the Union: —

No. 1.

Circular sent out by the Committee on Solar Research of the National Academy of Sciences.

To the President and Council of the.....Society.

Gentlemen,—The approach of the maximum period of solar activity, and the increasing interest in investigations of the Sun, have led to an inquiry regarding the possibility of establishing some plan of co-operation among individuals and institutions engaged in Solar Research. In order to ascertain whether a co-operative undertaking would receive general approval, a circular letter was sent to a number of investigators. The object proposed was an international

organisation, so planned as to interfere in no way with individual liberty, but rather to aid and suggest, wherever it could be of service, and to collect information for discussion. In all cases the replies were entirely favourable to the proposal, leaving no doubt that a carefully considered scheme would find universal acceptance. Among those who have already expressed their approval of the undertaking and their willingness to co-operate are: Professor A. Bélopolsky, Imperial Observatory, Pulkowa; Professor W. W. Campbell, Director of the Lick Observatory; Rev. A. L. Cortie, S.J., Stonyhurst College Observatory, Professor Henry Crew, North-Western University; M. Deslandres, Observatoire de Meudon; J. Evershed, Esq., Kenley, Surrey, England; Rev. Julius Fényi, S.J., Director of the Haynald Observatory, Kalocsa, Hungary; Professor George E. Hale, Director of the Yerkes Observatory; Professor B. Hasselberg, Royal Academy of Sciences, Stockholm; Sir William Huggins, President of the Royal Society, London; Professor H. Kayser, Director of the Physical Institute, Bonn; Dr. S. P. Langley, Secretary of the Smithsonian Institution; Sir Norman Lockyer, Solar Physics Observatory, London; M. M. Loewy, Director of the Paris Observatory; Professor A. A. Michelson, Director of the Ryerson Physical Laboratory, University of Chicago; Professor A. Riccò, Director of the Royal Observatory, Catania; Rev. Walter Sidgreaves, Director of the Stonyhurst College Observatory; C. Michie Smith, Esq., Director of the Solar Physics Observatory, Kodaikanal, India; Professor H. H. Turner, President of the Royal Astronomical Society; Gilbert T. Walker, Esq., Meteorological Reporter to the Government of India and Director-General of Indian Observatories; W. E. Wilson, Esq., Daramona, Ireland; Professor H. C. Vogel, Director of the Astrophysical Observatory, Potsdam; Professor A. Wolfer, Director of

the Observatory, Zürich; Professor C. A. Young, Director of the Halsted Observatory, Princeton.

At its recent annual meeting the National Academy of Sciences appointed the following Committee on Solar Research: George E. Hale, Chairman; W. W. Campbell, S. P. Langley, A. A. Michelson, and C. A. Young. This Committee was authorised by the Academy to co-operate with other Committees and Institutions in the preparation of general plans for Solar Research.

After a careful consideration of the various questions involved, the Committee on Solar Research has reached the conclusion that the best results may be attained through the joint action of Committees representing various Societies and Academies, such Committees to include among their members, so far as possible, the most active solar investigators and spectroscopists of all countries. It is important to note that physicists, as well as astronomers, should be represented on the various Committees, since some of the most important questions for consideration, such as the adoption of a uniform system of solar spectrum wave-lengths, are primarily of a physical nature.

In the name of the National Academy of Sciences, we accordingly invite the.....Society to appoint a Committee on Solar Research, and to send one or more representatives of this Committee to a General Meeting to be held in September next, in connection with the International Congress of Science at the St. Louis Exposition. As several important Societies have already expressed their readiness to appoint Committees, there is reason to expect that solar investigators will be well represented. It is proposed at this meeting to adopt a definite plan of organisation, and to discuss in a preliminary way the details of a programme of observations and

other matters, such as standards of wave-length, which will require subsequent consideration by the various Committees before final adoption.

Trusting that the.....Society will join in this undertaking, I have the honour to be,

Yours very respectfully,

GEORGE E. HALE,
Chairman.

Pasadena, Cal.,
June 3, 1904.

No. 2.

SOCIETIES ORIGINALLY INVITED TO FORM PART OF THE UNION.

The following is the list of the Societies which received the original invitation, together with their replies:—

Society.	Reply.
Royal Society of England	Accepts.
Paris Academy of Sciences	Accepts.
Vienna Academy of Sciences	Accepts.
St. Petersburg Academy of Sciences	Accepts.
Stockholm Academy of Sciences	Will Accept.
Amsterdam Academy of Sciences	Accepts.
Prussian Academy of Sciences	Declines.
Royal Astronomical Society of England	Accepts.
Society of Italian Spectroscopists	Accepts.
Astronomical and Astrophysical Society of America	Accepts.
Physical Society of France	Accepts.
Physical Society of Germany	Accepts.
American Physical Society	Accepts.
International Meteorological Committee	Accepts.
Astronomische Gesellschaft	
Astronomical Society of France	Accepts.
National Academy of Sciences	Accepts.

PART II.

Proceedings of the First Conference on Solar Research, held at St. Louis, September 23, 1904.

(Reprinted from the *Astrophysical Journal*, vol. xx., p. 301, December 1904.)

No. 3.

Minutes of the Meeting of Delegates to the Conference on Solar Research, held in the Hall of Congresses, St. Louis, September 23, 1904.

As Chairman of the Committee on Solar Research of the National Academy of Sciences, which had issued the call for the Conference, Professor George E. Hale called the meeting to order and explained the purpose of the Conference. After referring to previous movements to secure co-operation in solar research, he emphasised the importance of encouraging individual initiative, and urged that no less attention be paid to such encouragement than to the accomplishment of large pieces of routine work through co-operative effort.[1]

The following officers were then elected to serve for this meeting:—

President—Professor George E. Hale.
Vice-President—Professor Henri Poincaré.
Recording Secretary—Professor C. D. Perrine.

A motion that for this meeting the voting be by societies, and that each society be allowed one vote, was adopted.

[1] See *Astrophysical Journal*, December 1904.

A motion was made and carried that the chairman of each society's committee should cast the vote for that society.

After some remarks by Professor Hale on co-operation, the following motion was made by Professor Turner: —

> "That this meeting is in favour of the organisation of a scheme of international co-operation in solar research which shall encourage individual initiative, provide suggestions for definite lines of work, and facilitate the collection of results for publication."

This expression, after remarks by Professors Poincaré, Boltzmann, and Hale, was made the sense of the meeting.

Professor Hale stated that in appointing its Committee on Solar Research the National Academy of Sciences had given the Committee authority to join the President and Foreign Secretary of the Academy in presenting its plans for co-operation to the International Association of Academies. The opportunity now existed of securing the views of the Conference on this subject.

After remarks by various delegates upon the relations of scientific societies among themselves and to the International Association of Academies, the following motion was made by Professor Poincaré: —

> "That the committee to be appointed at this meeting negotiate with the Academy of Sciences of Vienna with the view of obtaining the approval and the patronage of the International Association of Academies."

This motion was seconded by Professor Boltzmann, and adopted.

A short discussion then ensued upon the fulness of the representation of interested individuals and societies

in the preparation of plans for co-operation in solar research.

Dr. Bauer moved:—

> "That the Sub-committee on Terrestrial Magnetism and Atmospheric Electricity of the International Meteorological Committee be invited to appoint a committee to co-operate with the Solar Research Committee."

This motion was seconded by Mr. Rotch, and adopted.

Professor Turner moved:—

> "That the Hungarian Academy of Sciences of Budapest also be invited to appoint a committee to co-operate with the Solar Research Committee."

This motion was seconded by Professor Frost, and adopted.

The following general motion was then made by Professor Poincaré and adopted:—

> "That the International Committee on Solar Research, to be appointed, be authorised to invite, at its discretion, societies and individuals which have been omitted, to co-operate."

The subject of the formation of an International Committee was then discussed.

Professor Turner moved:—

> "That each committee participating be invited to appoint a member to serve on the International Committee."

This motion was seconded by Professor Campbell, and adopted.

Mr. Rotch, delegate of the Sub-committee of the International Meteorological Committee on the relationship between meteorological and solar phenomena, reported on

the recent meeting of the Sub-committee at Cambridge. At that meeting the invitation of the National Academy of Sciences to co-operate with the other committees was accepted.

A short discussion followed on a provisional programme of observations, at the conclusion of which Professor Turner nominated the following gentlemen as a Committee to prepare a provisional programme, such Committee to have power to add to its number at discretion :—Professor George E. Hale, Professor Arthur Schuster, Professor Svante Arrhenius.

Suggestions from M. Deslandres as to observations to be undertaken, and the adoption of certain names of instruments and solar phenomena, were, at the suggestion of Professor Poincaré, referred to the Committee on Programme.

Following a discussion of routine measurements and computation, Professor Turner offered, on behalf of the Oxford University Observatory, to undertake a portion of this work.

A motion was therefore made by Mr. Abbot that the International Committee enter into negotiations with the Oxford University Observatory with the view of providing for the measurement of plates and the reduction of solar observations.

This motion was seconded by Professor Campbell, and adopted.

The suggestion was made by Dr. Bauer that if visual magnetic observations, especially magnetic observations at the eclipse of 1905, could be made at observatories with special instruments provided for the purpose, it was practically certain that the reduction of such observations would be undertaken by the Carnegie Institution.

A memoir on standards of wave-length, prepared by MM. Pérot and Fabry[1] on behalf of the Physical Society of France, was presented by Professor Poincaré.

A motion that standards of wave-length should be determined from terrestrial instead of solar sources was referred for consideration to the International Committee.

Professor Crew discussed the question of standards of wave-length, and pointed out the necessity of replacing Rowland's standards by a new system. He also presented a paper on this subject by Professor Kayser and a letter from Professor Michelson.

Mr. Jewell presented in abstract a paper on Rowland's standards.

Following is a list of committees and delegates appointed by various societies. Other societies will appoint committees in the near future:—

Society.	Committee.	Delegates Present at Conference.
Royal Society of England	Huggins, Christie, Lockyer, Schuster, Newall	Turner
Royal Astronomical Society of England	Turner, and others to be appointed	Turner
Astronomical Society of France	Deslandres, de la Baume Pluvinel, Guillaume, and others not yet named	Poincaré
French Physical Society	Poincaré, Fabry, Pérot, Deslandres, and others not yet named	Poincaré
German Physical Society	Ebert, Kayser, Kreusler, Lummer, Pringsheim, Runge, Straubel, Wilsing	
Amsterdam Academy of Sciences	Kapteyn, Julius	Kapteyn
Society of Italian Spectroscopists	Tacchini, Riccó, Cristoni, Mascari	
Vienna Academy of Sciences...	Hann, Weiss, von Lang, Exner	Boltzmann
St. Petersburg Academy of Sciences	Backlund, and others not yet named	Backlund
Subcommittee of International Meteorological Committee		Rotch

[1] See No. 6, p. 26.

Society.	Committee.	Delegates Present at Conference.
Stockholm Academy of Sciences		Arrhenius [1]
Astronomical and Astrophysical Society of America	Frost, Abbot, Bauer, Jewell, Perrine	Frost, Abbot, Bauer, Jewell Perrine
American Physical Society ...	Ames, Crew, Lewis, Mendenhall, E. F. Nichols	Crew, Mendenhall
National Academy of Sciences	Hale, Campbell, Langley, Michelson, Young	Hale, Campbell

A motion by Professor Poincaré was then adopted referring these communications to the International Committee.

Owing to the lack of time for discussion, the subject of total solar eclipses was referred to the International Committee, upon motion of Professor Campbell.

Oxford and Meudon were suggested as places for the next meeting. The International Committee was empowered to decide both the place and time of meeting, on motion of Professor Frost, seconded by Professor Poincaré.

Upon motion of Professor Poincaré, the thanks of the Conference were tendered to Professor Hale for his interest and activity in connection with the Conference.

The Conference then adjourned.

GEORGE E. HALE,
President.

C. D. PERRINE,
Recording Secretary.

[1] Acting informally.

PART III.

Papers submitted to the First Conference on Solar Research held at St. Louis, September 23, 1904.

(Reprinted from the *Astrophysical Journal*, vol. xx., p. 301, December 1904.)

No. 4.

Co-operation in Solar Research.[1]

By George E. Hale.

We have come together, gentlemen, in response to an invitation from a Committee of the National Academy of Sciences to consider the possibility of inaugurating a plan of international co-operation in solar research. Such a movement cannot be considered an entirely new one; for in the history of astrophysics we find records of several undertakings of a similar nature, some of which have yielded results of great importance. Foremost among these, perhaps, should be classed the important work of the Society of Italian Spectroscopists, which, through the observations of its own members and the effect of their example upon others, has added in a high degree to our knowledge of the Sun. We should also remember the important work inaugurated by Wolf and ably continued by his successor, Wolfer, at Zurich, of compiling Sun-spot numbers from observations made in all parts of the world. The well-known series of solar photographs discussed at Greenwich is another instance of a similar kind; for the negatives are secured through the co-operation of the Royal Observatory at Greenwich, the Solar Physics Observatory

[1] Introductory remarks of the chairman of the Committee on Solar Research of the National Academy of Sciences.

at Dehra Dûn, in India, and the Observatory at Mauritius. These photographs constitute the most important record of Sun-spot statistics now available. Indeed, they are so complete as to render similar work unnecessary, so far as the duplication of the extensive reductions go, on the part of other observatories. The initiation of this series of photographs is due to the Solar Physics Committee appointed by the British Government in 1879. In 1885 this Committee proposed to hold an international Conference in London to consider plans for co-operation in solar research; but through the inability of delegates to attend the Conference the project seems to have been abandoned. At the Congress held in Paris, in 1887, to prepare plans for making the International Photographic Chart of the heavens, there was some discussion of inaugurating co-operative investigations of an astrophysical nature; but hitherto they have not been carried into effect. More recently, a Solar Commission appointed by the Astronomical Society of France recommended that a general plan of solar research be undertaken. So far as I am aware, however, the work of this Solar Commission has hitherto been confined to France. At the recent meeting of the British Association in Cambridge a Sub-committee appointed by the International Meteorological Committee met to consider "the combination and discussion of meteorological observations from the point of view of their relations with solar physics." The principal object of this movement, as will be seen from the announcement of the purpose of the Committee, is to deal with meteorological questions related to solar phenomena. In the consideration of purely solar questions, I am happy to say that this Committee will co-operate with the other Committees represented at the present Conference.

From this brief, and avowedly incomplete, historical sketch

it will be seen that the movement we are inaugurating to-day can lay no claim to novelty. It is merely the culmination of a natural series of events, each of which has tended to emphasise the importance of solar research and the desirability of harmonious co-operation among the investigators at work in this field. I have had frequent occasion to point out the great possibilities which lie open to the observer of solar phenomena. It may be said without exaggeration that no department of science, so far as I am able to judge, offers more promising opportunities for important developments. In almost all cases solar observations are now being made with the methods and apparatus of a quarter of a century ago, and the powerful instruments of modern times, both astronomical and physical, have been but little applied in solar research. I do not mean that this is a rule without exceptions, but it nevertheless applies in a large proportion of cases. There can be no question that great advances in our knowledge of the Sun merely await the application of instruments and methods already available, but hitherto rarely employed.

So far I have spoken with special reference to observations of solar phenomena, but there are other questions of general spectroscopy which should play a no less important part in the deliberations of this Conference. No one who is acquainted with the progress of spectroscopy during the last twenty-five years in the physical laboratory can deny that its methods and its problems should be considered at the same time with the problems of solar physics. Within a very short time laboratory experiments on the phenomena of anomalous dispersion have yielded results so striking as to lead certain investigators to interpret solar observations on an entirely new basis. Although most solar physicists will doubtless hesitate to accept the radical

changes in solar theory which have been advanced, they must nevertheless be prepared to give careful consideration to the new claims. I might continue by pointing to many other phenomena of the laboratory, which must be studied in the closest union with solar questions; but this is needless before the present company. One question of the greatest importance—that of standard wave-lengths—will be fully treated by Professor Crew, and therefore need not be discussed by me. In my opinion, if I may be permitted to express it, the discussion of this and other similar subjects at the present Conference should be regarded as preliminary, subject to further consideration by the various Committees before final action is taken.

Two fundamentally different views are entertained among astronomers regarding the desirability of inaugurating schemes of co-operation in research. On the one hand, there are those who strongly favour co-operative undertakings, on the ground that through the intelligent distribution of work, which such a scheme assumes, large investigations can be carried out with the minimum expenditure of time and effort. Many who hold this view frankly assume that the participants in a co-operative undertaking are to perform their respective parts like so many machines, following a hard-and-fast programme from which no deviation can be permitted. This system involves the existence of a central bureau which exercises control over all of the work, and, in some cases, carries out the extensive reductions incidental to the research. This view is opposed by a large number of investigators, who think that the future of science depends in far greater degree upon the development of new ideas and the encouragement of individual genius than upon the accomplishment of any piece of work, no matter how extensive it may be. They urge, and with strong reason, that those

who engage in a co-operative undertaking, the lines of which have been sharply defined in advance, leaving no option to individual participants, must inevitably suffer, in so far as their originality of thought is concerned. Thus, while admitting that important results, perhaps not otherwise obtainable, can be secured by co-operation among individuals and institutions, they maintain that the loss will be greater than the gain in case the effect of the domination of some central authority is such as to stifle individual effort and prevent the development of new ideas and methods.

During the last few years these two phases of the subject have received much discussion in connection with many co-operative undertakings in science, variously organised and developed, and playing an increasingly important part in modern research. In spite of objections urged by some of the ablest investigators, these co-operative undertakings are actually being carried out; some with marked success, and others with less promise as to their future outcome. It must not be forgotten that in different departments of science the nature of the work varies so greatly as to make the question of co-operation apply in very different ways in different cases. In meteorology, for example, it is obviously essential that a great number of institutions should make routine observations at appointed hours and upon a carefully arranged plan, in order that the results may be suitable for systematic reduction and study. The same may be said of many other classes of work, particularly those which involve routine observations of secular phenomena. But it will be recognised that even in such a case there is nothing to preclude the development of new ideas and methods by those who are taking part in such a co-operative investigation. The danger, if any danger exists, is that the individual observers may be

allowed to believe that the instruments and methods they are called upon to employ represent the highest possible development, and that any deviation from the scheme must stand in the way of the results it is desired to attain.

In the history of science, instances are not lacking of co-operative undertakings which have dealt injury to participants. Instead of encouraging the development of new ideas, they have allowed the entire stress to be laid upon the accomplishment of given pieces of work according to a strict routine, and, either directly or indirectly, they have certainly discouraged individual effort, and thereby hindered progress. It is with a full appreciation of this fact that I approach the subject of co-operation in solar research. While I believe unquestionably that a science like our own cannot accomplish the most important advances without the collection of extensive data, beyond the reach of any individual or institution, I nevertheless feel that the future of the science depends in a much higher degree upon the encouragement of individual initiative. In suggesting, with others, the desirability of adopting some general programme of solar research, in which the observers and instruments in various parts of the world may find a part, I do so only on the supposition that such a movement need involve no discouragement of individual effort. Indeed, I would say more than this. I am not in favour of entering upon a co-operative undertaking unless it can be so planned as actually to encourage individual effort through the provision of conditions which would otherwise be lacking. Specifically, I believe that one of the principal objects of establishing committees on solar research should be the encouragement of little-known investigators to bring forward and develop ideas that may have originated with them. It may frequently happen that the special

atmospheric or instrumental conditions required in the accomplishment of a new piece of work may be readily available at some existing observatory, where they could be placed at the disposal of an investigator who might employ them in the realisation of his ideas. Through the work of the various national Committees, such cases would easily become known, and, in many instances, important new ideas might thus be advanced which would otherwise remain unkown.

At the very outset of the present undertaking, therefore, I would propose that the encouragement of individual ideas be regarded as one of the most important functions of any organisation we may establish. But, while attaching so much weight to this phase of the subject, I see no incompatibility between the encouragement of original ideas and the accomplishment of certain pieces of work which can best be done through co-operation. For example, let us take the observation of the spectra of Sun-spots. No one who has observed the spectrum of a Sun-spot and attempted, map in hand, to identify and record all of the widened lines, is likely to be so ambitious as to desire to include the entire length of the spectrum in his programme of daily observations. To secure adequate knowledge of Sun-spot spectra, it seems to me essential that practically all of the lines affected in the spots should be recorded. Those who share this view would undoubtedly be content to confine their attention to certain limited regions of the spectrum. At the time of the Sun-spot maximum, when many spots must be observed, it is obvious that the extent of the spectrum covered by one observer should not be too great. Furthermore, on account of interruptions from bad weather and the desirability of securing a check on the observations, it is equally obvious that the same region of the spectrum

should be observed regularly at more than one institution. Thus our knowledge of the spectra of Sun-spots would undoubtedly be materially increased if an understanding were reached as to the division of the spectrum among different investigators. From my knowledge of the work of this kind now in progress, or contemplated, I would anticipate no difficulty in securing such a division of labour. I can see no reason why a mutual understanding on this subject could be otherwise than advantageous to all who are interested in solar research.

In this reference to an important department of solar physics, which has been sadly neglected up to the present time, I have purposely confined my remarks to pure routine observations, such as are involved in the recording of the widened lines. But in this same field of research there is almost unlimited opportunity for individual investigations, many of which might involve the application of new and original ideas upon the part of those who pursue them. Hitherto, strangely enough, little or no attention has been given to the quantitative side of this subject, and few records of the carefully measured positions of widened lines can be found in the literature of astrophysics. Thus we have no knowledge of any such displacement of lines as may be produced by pressure or by the bodily motion in the line of sight of the gas which gives them rise. Line-distortions, it is true, have sometimes been measured; but my present reference is to the less violent motions which may possibly affect the entire mass of gas within the Sun-spot. There is large opportunity, also, for observations of the fine lines into which Young resolved the spot-band, as well as the interesting reversals so recently recorded by Mr. Mitchell, working under Professor Young's direction at Princeton. What I have said relates simply to visual observations,

which must continue to play a very important part in the study of spot-spectra. There is a great opportunity, however, for the development of photographic methods in this connection, and here, again, there will be abundant opportunity for the individual explorer.

This single instance will serve at once to illustrate the desirability of co-operation in solar work and the possibility of securing a system of co-operation which will accomplish the desired purpose and yet leave the individual free to initiate new classes of work. Numerous similar instances might equally well be mentioned. In all cases the work should be encouraged in every possible way.

I have dealt at such length with this question because it is so frequently assumed that co-operative work must injure the individual observer. I believe, on the contrary, that a system can be devised which, through co-operative effort, will result in an increase in the number of original suggestions and a more rapid improvement of observational methods.

No. 5.

Remarks on Standard Wave-Lengths.[1]

By Henry Crew.

It is a matter of no little interest that the man who laid the foundations of mathematical astronomy and devised the corpuscular theory should also have been the first to give us an absolute measure of the wave-length of light. Newton's values, published in 1704, remained unsurpassed for more than a century.

In 1815, Fraunhofer employed his newly invented

[1] Introductory remarks on opening the discussion on this topic at the Solar Conference at St. Louis.

gratings to determine some of the principal solar lines; these remained standard for nearly half a century.

Kirchoff, in 1862, gave his beautiful prismatic map of the solar spectrum, a model of precision and care.

The first normal map of the solar spectrum was published by Ångström in 1868. It was based upon his own determinations of absolute wave-length and was printed on a scale such that one millimetre on the map indicates a difference of one-tenth-metre in wave-length. For twenty years this work stood in the front rank. But in the later eighties there began to appear measurements of both solar and metallic spectra which introduced an order of accuracy hitherto undreamed of. Thalén, in 1884, pointed out the numerous sources of error in Ångström's map and measures.

But the year 1888 marks the beginning of a new era. For it was at this date that Rowland put out the final edition of his superb photographic map of the solar spectrum, and it was in the same year that Kayser and Runge gave us their exquisite rendering of the spectrum of the iron arc, both of these maps being based upon the absolute wave-length of the D lines, which Bell had just completed with extraordinary care by the grating method. A short list of solar standards was published by Rowland at the same time.

I remember very distinctly the general feeling in the spectroscopic circle which I then knew that, with the publication of Rowland's map, accompanied by his relative wave-lengths, and Bell's absolute value, we were at last on solid ground where a stand of many years might be made.

On the contrary, in 1893, Michelson had—by a method as yet unimpeached—already translated the standard metre into terms of the red cadmium ray, and had

incidentally proved that Bell's value of the absolute wave-length was too large by something like two-tenths of an Ångström unit.

Since then Michelson's values have been confirmed as perfectly as possible—at least, without the introduction of a totally different method—by Fabry and Pérot, and Hamy.

In this same year, 1893, appeared Rowland's "New Table of Standard Wave-Lengths," including both metallic and solar lines, upon which Hasselberg has based his highly accurate series of metallic arc spectra, starting with chromium in 1894.

In 1895 Rowland began, with the first number of the *Astrophysical Journal*, the publication of what he modestly called a "Preliminary Table of Solar Spectrum Wave-Lengths," expecting, as he says, "to add to it and correct it for a term of years until I can publish a standard list of the lines of the solar spectrum with all the elements to which they belong." From the very beginning the wide divergence between this table and the more probably correct absolute values of Michelson was admitted. The relative values of the table appeared for the time being almost beyond criticism.

In 1896 Jewell [1] published his observations on the want of exact coincidence between solar and corresponding metallic lines, showing not only that the solar lines are displaced in general toward the red, but that the different lines of any one element are *differently displaced*. The effect of this was, using Mr. Jewell's own words, "to make the lines of the solar spectrum step down from the commanding position which they have occupied as standards of reference."

[1] *Astrophysical Journal*, vol. iii., p. 89, 1896.

For the next four years matters remained about as Jewell's paper left them.

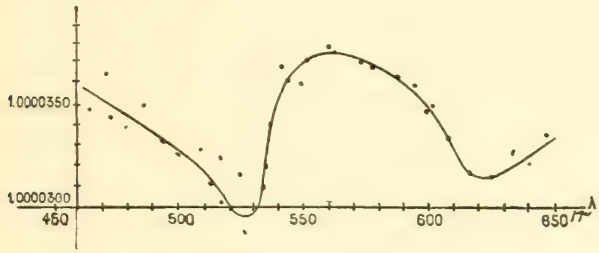
In 1900, however, Kayser discovered certain inconsistencies in Rowland's 1893 table, that is, the "New Table of Standard Wave-lengths." He found also that the distribution of iron lines in this table was such as to render it quite inadequate for purposes of accurate interpolation. To use Kayser's own words concerning a certain group of Rowland's iron standards: "My correction curves depending upon these lines always showed a quite impossible bend at λ3400, so that I was finally compelled to omit these standards."[1] These "impossible bends" in Kayser's curves refer to differences as great as 0·02 tenth-metre. Kayser at the same time published a fuller and revised list of iron standards, extending from λ2327 to λ4495, the relative values of which are based upon Rowland's metallic standards and are claimed to have a mean relative error not exceeding 0·003 tenth-metre. In other words, the *accidental* errors are much less than those of Rowland's "New Table of (Metallic) Standards."

The state of affairs at the time of Rowland's death (spring of 1901) may perhaps be described by saying that three shadows had fallen across the superb work embodied in his "Preliminary Table," namely, (1) the wide divergence from the correct absolute values, shown by Michelson; (2) the uncertainty attaching to solar lines, shown by Jewell; and (3) certain inconsistencies in relative values, shown by Kayser. I feel, however, that the figure is misleading; and with your permission I will change it and say that the subject had received illumination from three new sources.

In the year 1902 additional illumination was added from another brilliant source. I refer to the absolute

[1] *Astrophysical Journal*, vol. xiii., p. 331, 1901.

determination of thirty-three solar lines and fourteen iron lines by Pérot and Fabry, using the method of interference. These measures carried with them two important results, namely, (1) they proved that the relative values of the "Preliminary Table" were accurate, over small ranges, to one part in a million; in other words, the curve of errors in this table is a "smooth one"; (2) they proved that the relative values in this table deviate in each direction from the mean axis by as much as 0·02 tenth-metre, that is, by four parts in a million. In short, they proved that the absolute errors in the "Preliminary Table" extend to



the first decimal place, the relative errors over any considerable range extend to the second decimal place, while neighbouring lines are affected with relative errors only in the third decimal place. The serious consequences of this discoverey are realised only when we recall that for seventeen years past all the spectroscopic work of the world has been based upon this system which the two French physicists have shown to be affected with large systematic errors. In line-of-sight work, where 0·01 tenth-metre means 0·7 km. per second, the importance of these errors is enormous.

The upshot of the whole matter is that Rowland's relative values are too small from about the F line to the middle of the green, while from the middle of the green

to the D lines his values are too large by about the same amount, namely, an error ranging up to 0·02 Ångström unit.

The year 1902 thus brings us to the end of what may be called indisputable ground, which may be summarised by saying that Rowland's system, possessing these various defects, is still an instrument of great value, though less perfect than we formerly thought. Just how these errors have crept into the system is a matter of no little importance; indeed, it is a matter of very great importance, if we are to avoid similar errors in the future; at the same time, this is not the principal subject under discussion by this Conference. Hartmann has set forth [1] in a clear way the various sources of systematic and accidental errors in Rowland's tables. Michelson has predicted, and Kayser has verified, the fact that for gratings in general, the method of coincidences is not reliable even for relative values, except within limits which are much larger than was formerly believed. But the main question before us is not how the imperfections of the system arose, it is rather how the system may be perfected, with a minimum expenditure of time and energy, so as to answer the purposes of modern spectroscopy. Various suggestions looking toward this result have recently appeared. These would perhaps naturally form the subject of our discussion. So far as I am aware, the principal alternatives may be summarised as follows in terms of the accompanying correction curve, which Fabry and Pérot published in the *Astrophysical Journal*, Vol. xv., 272, 1902:—

[1] "A Revision of Rowland's System of Wave-Lengths," *Astrophysical Journal*, vol. xviii., p. 167—190, October 1903. Reprints of this article were sent by Professor Hartmann for distribution among those participating at the Solar Conference, as containing the expression of his views on the subject.

1. Correct to the axis of λ as rapidly as determinations of the grade of those by Michelson, and Pérot and Fabry will allow.

2. Reduce to a line drawn *parallel to the axis* of λ at such a distance that the sum of Rowland's errors will be zero.

3. Reduce to a straight line *intersecting the axis* of λ at such an angle as to make the sum of the squares of Rowland's errors a minimum.

These alternatives leave untouched the question of extending the corrections to the ultra-violet region.

No. 6.

Rapport sur la nécessité d'établir un nouveau système de Longueurs d'Onde Etalons.[1]

Par A. Pérot et Ch. Fabry.

La Société Française de Physique, comme suite à la communication de la National Academy, a nommé une commission chargée d'étudier la question de l'établissement d'une échelle de longueurs d'onde. C'est sur l'invitation de cette commission que nous avons préparé le présent rapport pour le Congrès International de l'Exposition de Saint Louis.

Nous avons, dans les lignes suivantes, cherché à exposer le plus clairement possible la nécessité de l'établissement d'une échelle de longueurs d'onde, à montrer que le spectre solaire devait être abandonné comme étalon, et à définir l'unité absolue.

La mesure des longueurs d'onde, outre qu'elle est nécessaire aux chimistes pour charactériser les corps par leur spectre d'émission, offre des moyens uniques d'investigation, aux phyisciens en particulier pour l'étude de l'émission lumineuse et de la constitution des radiations, aux astronomes pour la mesure des vitesses radiales des astres et la recherche des phénomènes qui se passent à la surface de notre soleil; mais, pour que ces études aient une base certaine, il faut que les longueurs d'onde soient mesurées avec toute la précision voulue, et il est intéressant

[1] Présenté au nom de la Société Française de Physique.

au point de vue international d'adopter une unité définie qui, une fois choisie, ne devra plus varier.

Dans les recherches spectroscopiques, on est amené généralement à faire des mesures relatives, c'est-à-dire à comparer la longueur d'onde de la radiation étudiée à celle d'une radiation connue; s'il est possible avec certaines méthodes, telle que la méthode interférentielle, de comparer directement sans aucun intermédiaire les longeurs d'onde de deux radiations quelconques, la méthode employée d'une manière courante, qu'elle repose soit sur l'emploi de prismes, soit sur celui de réseaux, ne permet que la détermination du rapport des longueurs d'onde de raies relativement voisines. Il résulte de là que la détermination d'une seule longueur d'onde serait insuffisante pour les applications, et qu'il faut, si l'on veut faire œuvre utile, mettre entre les mains des spectroscopistes les valeurs d'une série de longueurs d'onde convenablement choisies, déterminées par des méthodes instituées spécialement à cet effet avec toute la précision requise. Il en est de même dans d'autres travaux, tels par exemple que le nivellement du sol: loin de rapporter l'altitude d'une station quelconque directement au niveau de la mer, on relie usuellement l'altitude de cette station à celle d'une autre station déjà connue qui sert de base auxiliaire. On voit donc que, s'il est indispensable de déterminer les sources étalons de manière que les radiations émises puissent toujours être produites identiques à elles-mêmes, il est nécessaire également de fixer les valeurs des longueurs d'onde d'un nombre suffisant de points de repère convenablement choisis.

L'établissement d'un exact système de longueurs d'onde est d'une importance capitale, puisque toute erreur sur ces longueurs d'onde fondamentales se reportera dans toutes les mesures spectroscopiques. Il serait désirable que les

erreurs contenues dans le système fondamental fussent inférieures ou au plus égales aux erreurs accidentelles qui peuvent résulter des mesures par comparaison. Or il est à peu près certain qu'il n'en est pas actuellement ainsi : les mesures par comparaison peuvent être faites, avec les puissants appareils dispersifs dont on dispose actuellement, avec une précision qui dépasse le millionième ; telle serait la précision des mesures si les valeurs des longueurs d'onde des raies de comparaison avaient la même précision : or celle de l'échelle adoptée est beaucoup moindre ; aussi, M. Kayser pouvait-il récemment émettre l'opinion qu'une grande partie des désaccords entre les mesures faites par divers observateurs sur une même raie provenait de l'emploi de valeurs incorrectes pour les longueurs d'onde des raies de comparaison.[1]

Depuis les admirables travaux de Rowland, c'est-à-dire depuis environ 15 ans, tous les travaux de spectroscopie ont été faits en prenant comme point de départ les nombres donnés par ce savant. On pouvait cependant déjà prévoir une difficulté dans l'application de ce système de longueurs d'onde : les mesures de Rowland ont été faites sur les spectre solaire, il faudrait donc, pour opérer d'une manière correcte, prendre le spectre solaire comme spectre de comparaison, c'est ce que peut-être, aucun observateur n'a fait ; on s'est servi de spectres de métaux, en admettant que les longueurs d'onde son les mêmes que dans le spectre solaire ; or l'exactitude de ce postulat est de plus en plus improbable. Il est vrai que certaines raies

[1] H. Kayser, *Astrophysical Journal*, vol. xix., p. 158, 1904 : "With a correct system of standards we could now determine the wave-lengths of all the sharp lines—and 99 per cent. of all the lines can be got sharp—with an accuracy of a few thousandths of an Angström unit. I am sure that the much larger differences found by different observers are caused very often by the use of different, relatively incorrect, standards."

métalliques, produites par l'arc électrique ont été spécialement mesurées par Rowland, mais ces mesures ne méritent pas la même confiance que celles des raies du spectre solaire; Rowland était convaincu de l'identité absolue des longueurs d'onde dans le spectre solaire et dans celui de l'arc électrique, et lorsque des écarts se manifestaient entre les deux espèces de spectres, il les attribuait à un déplacement de la plaque photographique ou à un défaut de réglage des faisceaux, et il cherchait par des corrections convenables à faire disparaître les écarts dans les résultats.[1] En employant les nombres de Rowland pour des raies de métaux dans l'arc électrique, on emploie donc ou bien des nombres obtenus en mesurant autre chose que ce que l'on emploie, ou bien des nombres un peu incertains; il faudrait se résoudre, si l'on voulait profiter le mieux possible des travaux de Rowland, à employer uniquement le spectre solaire comme spectre de comparaison.

Mais les valeurs données par Rowland pour les longueurs d'onde du spectre solaire sont-elles du moins parfaitement correctes? L'auteur pensait qu'elles étaient exactes au millionième environ. La meilleure vérification consistait à reprendre les mesures d'un certain nombre de raies du spectre solaire lui-même, par une méthode aussi différente

[1] Rowland, *Physical Papers*, p. 564 : "In every plate having a solar and metallic spectrum upon it, there is often—indeed always—a slight displacement. This is due either to some slight displacement of the apparatus in changing from one spectrum to the other, or to the fact that the solar and the electric light pass through the slit and fall on the grating differently. In all cases an attempt was made to eliminate it by exposing on the solar spectrum, both before and after the arc, but there still remained a displacement of 1/100 to 2/100 division of Angström, which was determined and corrected for by measuring the difference between the metallic and coinciding solar lines, selecting a great number of them, if possible."

Voir aussi : L. E. Jewell, *Astrophysical Journal*, vol. iii., p. 89, 1896.

que possible de celle de Rowland, et autant que possible plus directe. C'est ce que nous avons fait [2] en comparant directement, par une méthode interférentielle, un certain nombre de raies du spectre solaire avec une même raie du cadmium. Nos mesures ont porté sur la partie du spectre comprise entre les longueurs d'onde 464 et 647.

La comparaison de nos résultats avec ceux de Rowland conduit aux résultats suivants :

1. Il n'existe pas, dans les tables de Rowland, d'erreurs accidentelles atteignant le millionième; il est même probable qu'au point de vue des erreurs accidentelles, les nombres de Rowland sont au moins aussi précis que les nôtres. Si l'on prend dans les tables de Rowland deux radiations très voisines, les rapports des nombres donnés pour les longueurs d'onde sont toujours parfaitement exacts.

2. Il existe dans les mêmes tables des erreurs systématiques notables (atteignant presque le centmillième); ces erreurs varient d'une façon régulière en fonction de la longueur d'onde. Si donc on prend dans la table de Rowland les longueurs d'onde de deux radiations situées d'une manière quelconque, le rapport des deux nombres peut être erroné presque de 1 centmillième, c'est-à-dire d'une quantité dix fois plus grande que les erreurs que l'on peut commettre dans les mesures de comparaison.

L'exactitude de nos résultats a donné lieu au début, à quelques contestations, elle semble aujourd'hui générale-

[2] Ch. Fabry et A. Pérot, "Mesures de longueurs d'onde en valeur absolue; spectre solaire et spectre du fer," *Annales de Chimie et de Physique*, Janvier 1902; et *Astrophysical Journal*, vol. xv., p. 73, 261, 1902.

ment admise.[1] On ne voit d'ailleurs pas comment des erreurs systématiques, fonctions de la longueur d'onde, auraient pu s'introduire dans nos mesures.

Il y aurait donc lieu de reprendre les mesures de Rowland. Les erreurs n'étant que systématiques, et régulièrement variables avec la longueur d'onde, on pourrait se proposer de construire une table de correction, analogue à celle que nous avons donnée, mais plus étendue, de manière à profiter complètement du travail de Rowland; mais il ne faudra pas, dans ce cas, perdre de vue que les nombres de Rowland ne s'appliquent qu'au spectre solaire, et si l'on veut avoir les longueurs d'onde de raies métalliques, il faudra les comparer directement et sans idée préconçue aux raies solaires. On peut se demander s'il ne serait pas plus sûr, et presque aussi simple, de reprendre complétement le travail de la détermination des étalons. Nous allons donc envisager les diverses solutions possibles, en supposant le travail repris par la base.

Choix des radiations.—La première question à résoudre paraît être celle-ci: Convient-il, pour définir l'échelle des longueurs d'onde, d'adopter des radiations empruntées au spectre solaire, ou des radiations d'origine artificielle?

[1] Voir à ce sujet :

Louis Bell, "On the Discrepancy between Grating and Interference Measurements," *Astrophysical Journal*, vol. xv., p. 157, 1902.

A. Pérot and Ch. Fabry, "A reply to the Recent Article by Louis Bell," *ibid*, vol. xvi., p. 36, 1902.

Louis Bell, "The Pérot-Fabry Corrections of Rowland's Wave-Lengths," *ibid*, vol. xviii., 191, 1903.

Fabry et Pérot, "On the Corrections to Rowland's Wave-Lengths," *ibid*, vol. xix., p. 119, 1904.

G. Eberhard, "Systematic Errors in the Wave-Lengths of the Lines of Rowland's Solar Spectrum, *ibid*, vol. xvii., 141, 1903.

J. Hartmann, "A Revision of Rowland's System of Wave-Lengths," *ibid*, vol. xviii., 167, 1903.

H. Kayser, "On Standards of Wave-Lengths," *ibid*, vol. xix., p. 157, 1904.

C'est la première de ces solutions que l'on a adoptée au début de la spectroscopie (Ångström), et que Rowland a conservée. Il est certain que l'emploi du spectre solaire a l'avantage de dispenser l'observateur de tout soin à donner aux sources de lumière, et que le nombre immense des raies de son spectre offre dans certains cas des avantages; ce grand nombre de raies était à peu près nécessaire pour l'emploi de la méthode des coincidences de Rowland. Mais l'emploi de ce spectre offre des inconvénients qui contrebalancent, et bien au delà, ces avantages : en dehors des altérations régulières le longueur d'onde, produites par la rotation du soleil et par les mouvements relatifs réguliers de la terre et du soleil, altérations dont on tient facilement compte, la chromosphère solaire est le siège de mouvements violents; d'autres causes mal connues peuvent agir, et l'on a des exemples de changements, momentanés il est vrai, mais extrêmement importants de ce spectre.[1] Si les nouvelles théories solaires de M. Julius [2] se confirmaient, de petites variations de longueurs d'onde des raies solaires n'auraient plus rien de surprenant. En outre comme nous l'avons fait déjà remarquer, le spectre solaire n'est jamais employé comme spectre de comparaison, pas plus par les astronomes que par les physiciens; on se sert toujours de radiations artificielles, qui ne peuvent être que des étalons secondaires

[1] Thollon, *Annales de l'Observatoire de Nice;* Hale, *Astrophysical Journal*, vol. xvi., p. 220, 1902.

[2] W. H. Julius, "Solar Phenomena, considered in connexion with Anomalous Dispersion of Light," *Astrophysical Journal*, vol. xii., p. 185, 1900.

"Peculiarities and Changes of Fraunhofer Lines interpreted as consequences of Anomalous Dispersion of Sunlight in the Corona," *ibid.*, vol. xviii., p. 50, 1903.

"Les théories solaires et la dispersion anomale," *Revue générale des Sciences*, vol. xv., p. 480, 1904.

si le spectre solaire est pris comme spectre fondamental. L'emploi de raies sombres pour la définition d'une longueur d'onde est peut-être moins simple et moins avantageux que l'emploi de raies brillantes. Enfin, la mesure de la longueur d'onde d'une raie sombre par les méthodes interférentielles, qui seront sans doute employées dorénavant pour les mesures fondamentales, est beaucoup moins facile et un peu moins précise que la mesure d'une raie brillante.

Si l'on se décide à abandonner le spectre solaire pour l'établissement de l'échelle fondamentale des longueurs d'onde, on est forcément amené à prendre des sources de lumière artificielles donnant des raies brillantes (gaz rendu lumineux par des procédés convenables).

Les radiations choisies doivent satisfaire aux deux conditions fondamentales suivantes :

1. Il faut se mettre à l'abri de toute variation possible de la longueur d'onde, et pour cela définir exactement toutes les circonstances qui définissent l'état du gaz et la manière dont il est rendu lumineux. Cela exige certainement quelques précautions, et sur certains points de nouvelles études sont désirables ; mais on peut affirmer dès maintenant qu'il est possible d'avoir une constance absolue de certaines longueurs d'onde. En tout cas l'emploi de sources artificielles présente sur l'emploi de la lumière solaire cet avantage immense que l'on peut expérimenter sur elles, tandis que sur la lumière solaire, on ne peut qu'observer.

2. Il faut que les raies brillantes employées comme étalons soient suffisamment fines, de manière à définir une longueur d'onde bien déterminée. Il faudra éviter de se servir de raies accompagnées de satellites, à moins qu'ils ne soient très faibles, de telle sorte que la longueur moyenne ne diffère pas de celle de la composante principale,

ou qu'ils ne soient suffisamment écartés pour que l'on puisse employer l'une des composantes bien déterminée.

On peut dès à présent affirmir que les spectres de certains métaux dans l'arc électrique donneront un grand nombre de raies satisfaisant à toutes les conditions requises.

Les étalons primaires ainsi choisis pourraient ne pas être extrêmement nombreux; quelques dizaines dans le spectre visible et ultra-violet suffiraient probablement; il serait facile de leur rapporter par interpolation toutes les autres raies que l'on jugerait commode d'employer comme étalons secondaires. Quant au spectre solaire, son étude rentrerait dans le domaine de l'astronomie physique, comme un moyen extrêmement puissant pour l'étude des phénomènes solaires.

Quant à la question de détermination de la longueur d'onde de ces radiations étalons, elle semble *a priori* pouvoir être faite suivant une unité de longueur arbitraire; on pourrait songer à prendre par exemple la longueur d'onde de la raie rouge du cadmium égale à l'unité; le nombre caractérisant une longeur d'onde serait alors le rapport de cette longueur d'onde à la longueur d'onde du cadmium, mais il semble bien préférable d'adopter une unité rattachée directement au système métrique; les comparaisons des longueurs d'onde entre elles comportant, semble-t-il, des erreurs du même ordre que celles qui ont pu être commises dans la mesure de la longueur d'onde de la raie rouge du cadmium faite par MM. Michelson et Benoit; ces dernières erreurs, si elles existent, n'apparaîtraient pas; la mesure en valeur absolue d'une seule longueur d'onde suffit d'ailleurs puisque l'on peut, sans difficulté, par la méthode interférentielle, comparer la longueur d'onde d'une radiation quelconque à celle de la radiation choisie comme étalon primaire. C'est, du reste, ce que nous avons fait dans tous nos travaux sur ce sujet,

et une longue pratique nous a montré que l'emploi des raies du cadmium ne présente pas de difficultés, et définit une longueur d'onde avec une précision qu'il sera bien difficile de dépasser. Nous avons déjà donné les longueurs d'onde d'un certain nombre de raies du fer comparées de cette manière à la raie fondamentale du cadmium.[1] Tout récemment M. Kayser a préconisé la même solution, et a annoncé que des mesures de ce genre étaient en cours dans son laboratoire.[2]

M. Hartmann a proposé récemment[3] de choisir une unité de longueur telle que les nombres de Rowland soient le moins possible altérés; en d'autres termes, les rapports entre les nombres de Rowland n'étant pas exacts, on ne peut dire qu'ils soient rapportés à une unité définie quelconque, mais on peut chercher une sorte d'unité moyenne, telle que ces nombres soient altérés le moins possible; M. Hartmann a calculé une table de correction, simple transformation numérique de la nôtre, qui est basée sur cette condition. Il est évident que si, comme nous le proposons, on se décidait à reconstruire de toutes pièces une nouvelle échelle de longueurs d'onde, cette solution n'aurait acune raison d'être. D'ailleurs pour que l'énorme travail de Rowland ne soit pas perdu (on sait que la *preliminary table* contient environ 20,000 raies), il suffirait de comparer par interpolation quelques raies solaires avec les étalons fondamentaux, et de construire une table de corrections plus étendue et peut-être plus exacte que celle que nous avons donnée.

Influence de l'air.—Les longueurs d'onde doivent être définies par leurs valeurs dans l'air. Les valeurs absolues étant très notablement affectées par les variations de

[1] *Ann. de Chim. et de Phys.*, Janvier 1902, et *Astrophysical Journal*, vol xv., 73, 261, 1902.

[2] *Ibid.*, vol. xix., 157, 1904. [3] *Ibid.*, vol. xviii., 167, 1903.

température et de pression, il convient, comme l'ont fait MM. Michelson et Benoit, de définir soigneusement ces conditions pour les mesures absolues; mais les rapports des longueurs d'onde sont très peu affectés par les variations atmosphériques, à cause de la faible dispersion de l'air. Cependant, dans des conditions extrêmes de température et de pression, et pour les extrémités du spectre, les variations des rapports des longueurs d'onde peuvent porter sur les millionièmes. Il y a donc lieu dans la définition de l'échelle des longueurs d'onde de dire à quelle température et à quelle pression elle a été établie, mais une définition assez grossière de ces conditions suffit. Dans les mesures par interpolation, il n'y a jamais à tenir compte des conditions atmosphériques.

La solution qui s'impose consiste donc à adopter comme étalon fondamental une raie du cadmium sous certaines conditions de température et de pression, produite dans des conditions rigoureusement déterminées, connue en valeur absolue, grâce aux mesures de MM. Michelson et Benoit.

Les conclusions du présent rapport sont donc:

I. Il y a lieu d'établir une échelle nouvelle de longueurs d'onde étalons.

II. Ces longueurs d'onde seront celles de radiations dues à des sources artificielles, parfaitement définies, et susceptibles d'être reproduites toujours les mêmes.

III. Elles seront mesurées par des expériences spéciales faites par différents expérimentateurs relativement à la radiation rouge du cadmium, produite par le passage d'un courant alternatif ou d'une décharge de bobine dans un tube de Michelson à électrodes d'aluminium dont le tube capillaire a 1 cm. de longueur et 2 mm. de diamètre.

IV. Provisoirement, et jusqu'après de nouvelles expériences, la longueur d'onde de la raie rouge du

cadmium ainsi définie sera considérée comme égale à 6438·4722, dans l'air à 15° sous la pression de 760 mm. de mercure.

V. Parallèlement à ce travail, on déterminera une courbe de corrections relatives aux mesures de Rowland.

Laboratorie d'Essai
du Conservatoire National des
Arts et Métiers, Paris.
Université de Marseille.

No. 7.

New Standards of Wave-Length.

By H. Kayser.

For many years spectroscopists had to be content with the determinations made by Ångström for the visible part, and extended later by Cornu to the ultra-violet part as far as λ3000. For the wave-lengths to λ2200 Liveing and Dewar and Hartley gave the first measurements. At that time errors between 1 and 5 Ångström units were not infrequent, *i.e.*, wave-lengths could be determined to only about one one-thousandth of their value. An immense advance was made by Rowland. He no longer tried to determine absolutely the wave-lengths of a great number of lines by the grating, but, applying the much more accurate method of coincidences, found the values of some hundred lines between λλ2000 and 7600, relative to a value of the D lines taken somewhat at random from the absolute measurements by Bell, Kurlbaum, Müller and Kempf, and Peirce. The method of coincidence, as is well known, is founded on the theory of the concave grating, by which

at the same point lines of wave-length λ in the first order, of $\frac{1}{2}\lambda$ in the second order, of $\frac{1}{3}\lambda$ in the third, and so on, are brought to focus.

Rowland expressed the opinion that none of his standards could deviate by a hundredth of an Ångström unit from the correct value, and this seemed probable when one considered the first-rate instruments used by Rowland, and the eminent ability of Rowland and his assistant Jewell for such work.

This faith received the first shock when Michelson by his beautiful interferometer method determined at Paris, in collaboration with Benoit, the absolute values of three *Cd.* lines. Not only the absolute values were greatly different from Rowland's, but the relative values also showed discrepancies amounting to more than 0·03 Ångström units. Rowland's system received a much severer blow, when Professors Pérot and Fabry by another kind of interferometer measured many solar and iron lines in the visible spectrum and showed that Rowland's system contained values alternately too high and too low. It is now quite certain that Rowland overestimated the correctness of his measurements, and that in using his numbers as standards we never can be sure of obtaining relatively exact numbers within 0·01 unit. Besides these fluctuating errors pointed out by Pérot and Fabry, many of Rowland's lines have individual errors amounting to 1 or 2 hundredths, as anyone may see by using different standards for the determination of the same line. Some years ago I tried to give a table of iron standards free from these individual errors, but of course my table, being based on Rowland's, is subject to nearly the same fluctuations of values.

For the, in practice, more important ultra-violet part of Rowland's system we have no evidence whatever of

correctness or incorrectness: but it is highly probable that it will contain errors of the same kind as the visual part.

If one tries to determine the wave-length of a sharp line—and nearly 99 per cent. of all spectrum lines can be obtained sharp—by interpolation between neighbouring standards, he will find it not difficult, by using a good concave grating and a good measuring engine, to get with a single measurement the value within five thousandths of a unit, if he uses the same standards; but using others he may get differences of one or two hundredths. So it appears that the uncertainty of our wave-length determinations is caused by the insufficiency of Rowland's standards.

To say a word on the importance of obtaining wave-lengths as correct as possible, for astronomical purposes, for the identification of elements, for the series relations, etc., seems needless. Thus the necessity of creating a new system of standards is evident, and it seems time to consider the way by which a better system may be produced and the errors committed by Rowland avoided.

One of Rowland's errors was the use of mixed solar and arc lines, which he thought were of identical wave-length. Since that time we know, by the publications of Jewell, Humphreys and Mohler, that they are different. This cause of error could thus be easily avoided, and it seemed as if, by applying the method of coincidences to arc lines only, a correct system could be produced. During the last twelve years I have three times tried to do so, but have always failed—I did not know why.

After the publication of Messrs. Perot and Fabry's measurements of iron lines I formed the plan of founding the method of coincidences on their measurements: it is much easier to get a system of standards throughout the spectrum by using many fundamental lines than by using

a single one. But as I was not sure of the absolute correctness of Pérot and Fabry's numbers, though they seemed very reliable, I felt obliged to convince myself. This can be done by the method of coincidences, as one line can be got by that method from others.

While engaged in this research there appeared a paper from Michelson showing that errors in the ruling of gratings are possible which make the method inapplicable: a line of given wave-length in the first order does not coincide with another line of half that wave-length in the second order. I immediately changed the scope of my investigations and made some tests to see if Rowland's gratings have such an error. I have two of Rowland's largest gratings ruled on his second and third dividing engines. The comparison of results obtained with them by the method of coincidences gave differences of more than 0·03 Ångström units, while repeated determinations with each grating alone agreed to a few thousandths.

It is thus apparent that Rowland's gratings are not suited for the application of this method, and it is probable that the larger part of the errors in Rowland's system are due to this cause. As gratings are therefore not to be used for exact absolute determinations, they are useless also for relative measurements, if great accuracy is desired. At present we have no better engines, though different American physicists are planning new constructions, as Michelson and Wadsworth. Time must show if their engines are better. We are now under the deplorable necessity of disregarding the method of coincidences in the creation of a new system of standards; the spectra of gratings can be used only for interpolation between standards not too far apart.

It seems to me, then, that the only way open consists in the determination of a great number of arc lines with

some interferometer, and probably Pérot's and Fabry's form of that instrument will prove most appropriate. Its use is not easy; it requires long practice even for the visible part of the spectrum, and for the ultra-violet part the difficulties will be much greater. Not a single observer, but several, should undertake this task, and when their numbers agree to one or two thousandths of a unit for, say, fifty or a hundred lines throughout the spectrum, then the grating may again be applied for getting by interpolation some hundred lines more. Then it will be possible to get all the spectral lines accurate to some thousandths of a unit, a great advance will have been made, and many questions that are now unapproachable can be answered.

Of course, all the measurements of spectra already made—and I have myself worked a good deal in that direction—will be useless as far as wave-lengths are concerned. Professor Hartmann, of Potsdam, thinks it already now possible to calculate from Pérot and Fabry's measurements a table of corrections, by the application of which Rowland's measurements, and all the others based on Rowland's standards, may be corrected. I think that is impossible. We do not even know that the fluctuations in Rowland's system are truly known, as very few lines have been re-measured by Pérot and Fabry, and they may have fallen on some lines wth large individual errors, and the curve of corrections may become quite another by the use of other lines. But even if that were not the case, who knows from what individual standards every line of a spectrum has been determined? Hasselberg is the only man who gives in his tables the standards between which he has interpolated, so that his numbers can be corrected when corrections to Rowland's standards are known. The greater errors of individual lines in Rowland's tables

cannot be corrected. And, further, it is quite impossible to deduce any table of corrections for the ultra-violet part. As the method of coincidences has broken down for gratings which have been tested, we can get no conclusion whatever as to the behaviour of Rowland's system in this part of the spectrum without new absolute measurements.

I therefore think Rowland's system must be entirely abandoned and a new system created. Since for all spectroscopic purposes only relatively correct values are needed, the new system may be based on Rowland's incorrect value for the D lines; then new measurements would coincide within some hundredths instead of tenths of a unit with the old ones. I think it more logical, if an entirely new system is to be created, to base it on Michelson's absolutely exact values of the *Cd* lines. But this question is of minor importance.

No. 8.

The revision of Rowland's system of Standard Wave-Lengths.

By Lewis E. Jewell.

In considering the desirability of revising Rowland's system of wave-lengths, and of changing to Michelson's absolute values, it is well to consider carefully what were the probable sources of error entering into the determination of Rowland's or Bell's absolute values, Rowland's "New Table of Standard Wave-Lengths," and Rowland's "Preliminary Table of Solar Spectrum Wave-Lengths." It is also desirable that we should consider what work bearing upon this subject has been accumulated capable

of furnishing a basis for more accurate tables; and what work it is desirable to do in the near future, in order that the whole subject may be placed upon a satisfactory basis, with the least friction and the least expenditure of energy. In the discussions which have been in progress for some time regarding the corrections to be applied to relative wave-lengths, and the relations between solar and metallic lines, there are some very important factors which have been almost completely ignored. In the first place, it is well to consider the various errors to which the wave-lengths of lines in the various tables published by Rowland were subject.

The absolute values of wave-length were derived from determinations by Bell, influenced in a measure by determinations of other observers. These were determinations of the wave-length of lines in the solar spectrum, and were corrected, approximately at least, for temperature and air-pressure. Since then better values for the refraction of the air, at different pressures and temperatures, have been determined. The measurements by Bell were also corrected for motion in the line of sight, of the observer's position, as caused by the Earth's rotation upon its axis and revolution around the Sun in a slightly eccentric ellipse.

The first determinations of wave-length used in Rowland's early tables were made by Rowland and Koyl, mostly from eye-observations, and were at best only approximate values, and many of the lines taken as standards were of an unsatisfactory character. These measurements were used in the preparation of Rowland's later tables, but were not given much weight, and some of the lines were discarded. Later eye-measurements of relative wave-length were made with both the concave and plane gratings by Rowland and Crew, and afterwards

by myself from photographic plates taken by Rowland with two or three concave gratings of different values for the grating space. These photographs were upon very fine-grained plates, and the definition in general was very good, but, except in a few cases, no data were given from which the corrections to be applied for temperature and pressure of the air could be derived. Also in the eye-measurements no corrections of this character were made. Rowland himself did not pretend that the measurements were accurate to less than one-hundredth of an Ångström unit, and, such being the case, he had little patience with the idea of making these small corrections; although it is likely that they were responsible for many errors, both systematic and accidental, in his tables, which at times might have been rather greater than one-hundredth of an Ångström unit.

Having made practically all of the measurements from the photographic plates, and the calculations in the work of reduction, I can speak more positively regarding them. They consisted, for the greater part, of two groups of plates, nearly all taken by Rowland, upon fine-grained emulsions prepared by himself. The first set comprised eighteen plates, 14 in. (35 cm.) long by $1\frac{7}{8}$ in. (4·8 cm.) wide, of overlapping spectra for the determination of relative wave-lengths by the method of coincidences, and consisted of a middle strip containing a portion of the solar spectrum in some particular order, and on each side of it strips of a portion of some overlapping spectrum of another order. These were taken by the aid of a shutter which in one position allowed the middle spectrum to reach the photographic plate, and when the shutter was rotated allowed the spectrum of the outside strips to reach the plate, the middle strip being then covered up by the shutter. Rowland was in the habit of giving one of these

strips two exposures, one both before and after exposing the other strip. He did this under the impression that, if anything happened to the apparatus between the first and second, or between the second and third exposures, the average of the first and third would equal the second exposure. This, however, by no means follows. If the effect that produces the error be of a continuous nature, such as the heating of the grating or slit during the exposure or some such cause, that might be true; but if it were due to the act of turning the shutter in some direction and then back again, or some mechanical movement or jar, this would by no means follow, and the third exposure added to the first might simply introduce an error where there had been none before. Also the first change in the shutter might introduce an error in one direction, and the return of the shutter to its original position might introduce an error in the opposite direction, bringing things back to the original condition; and the first and third exposures might thus very well be coincident, but the second exposure would not be compensated for by the average of the other two.

In photographs taken by myself I have found it better to see to it that the shutter worked easily and then to make only two exposures, turning the shutter and doing everything else to avoid any pressure against the apparatus in any direction, and to avoid jars of any kind to parts of the spectograph. There is no question whatever but that for such work the shutter should be disconnected from the spectrograph, and made entirely independent in its working, or that the parts should be carefully balanced, work easily, and be operated pneumatically or electrically, so that there can be no displacement due to motion of the shutter or pressure against parts of the apparatus. There is no doubt in my mind that in the plates taken

for the measuring of coincidences errors of this kind did probably occur, and at times may well have amounted to considerably more than one-hundredth of an Ångström unit. It is quite possible that the errors arising might be such as to cause the lines of the second exposure to be always shifted in a given direction with respect to the other two; and the errors might possibly differ as the camera of the spectograph was moved to different portions of the spectrum, for even a shift in the position of the observer will cause some variations.

In photographs containing solar and metallic arc-spectra upon the same plate, the solar spectrum was usually taken in the middle, and the arc spectrum outside. Some of these plates were taken by Rowland, and some of them by myself under Rowland's direction; but scarcely any of them contained data for the determination of motion in the line of sight, or for correcting for atmospheric pressure and temperature, where lines of different orders of spectra overlapped. Another remarkably fine set of plates of the solar spectrum alone were taken by Rowland for making his " Photographic Map of the Solar Spectrum "; but only one of them was used in these measurements, and scarcely any of them had data marked upon them.

The measurements upon both sets of plates mentioned were made by myself, nearly all of them upon a dividing engine constructed by Mr. Schneider under Rowland's direction. The screw of this dividing engine was carefully made by Schneider, and is remarkably free from errors, but the attached microscope was unsatisfactory. The definition of the plates for coincidences of overlapping solar spectra for the determination of relative wave-lengths were, in general, fairly good, and some of them excellent. The plates for the coincidence of solar and metallic lines were, some of them, good, some bad, and others indifferent.

Practically all of them were devoid of data for corrections. One source of trouble was experienced with photographs taken upon thick plates of uneven thickness from the varying focus of different parts of the plate and consequent parallax. Adjustment was made to avoid the trouble as far as possible, and later on means were adopted for nearly getting rid of the trouble. There was, however, always some parallax from curvature of the field of the microscope, but the trouble was avoided and allowed for, as far as possible. The plates for comparison of solar and metallic spectra were 19 in. (48 cm.) long by $1\frac{1}{4}$ in. (3·2 cm.) wide, and bent to the focal curve of the grating.

Notwithstanding the difficulties mentioned, measurements made upon good lines could, in general, be relied upon to two or three-thousandths of an Ångström unit, and some good lines to one-thousandth of a unit.

All measurements of relative wave-length where lines in different orders of spectra, or solar and arc spectra, were compared, were subject to the uncertainties due to motion in the line of sight and changes of wave-length due to air-pressures and temperatures, which were not allowed for. Also a few plates were of a very unsatisfactory character, but were used because no others were available at the time.

All of these causes produced errors in Rowland's published wave-lengths which were not allowed for in any way. Also I had discovered a systematic difference in the wave-length of lines in the arc and solar spectra, but Rowland could not be convinced that the difference was due to any cause other than a disturbance of the apparatus between the exposures of arc and solar spectra; and, as a consequence, he had the displacements for different lines upon a given plate averaged up, and applied this as a correction to the arc wave-lengths, to bring them into agreement with the corresponding solar wave-lengths.

This matter has since been thoroughly investigated by myself, and the cause of this difference in wave-lengths determined to be mostly motion in the line of sight of the matter in the Sun's atmosphere producing the absorption lines of the solar spectrum, and to a small extent to a difference of pressure in the atmosphere of the Sun and the electric arc.

For Rowland's "New Table of Standard Wave-Lengths," published in 1893, these measurements were used, together with all previous observations available, and weighted according to their probable accuracy. Then the results were arranged and worked over by Rowland, who introduced certain empirical corrections, where he deemed them necessary, and thus was constructed the table known as Rowland's " New Table of Standard Wave-Lengths." In 1896 Rowland published in the *Memoirs of the American Academy of Arts and Sciences* an extended account of the methods used by him in the construction of this table.

Later I measured up the complete solar spectrum upon Rowland's plates which were used for the production of " Rowland's Photographic Map of the Solar Spectrum." These plates have already been referred to as of particularly good quality.

Considerable difficulty was experienced during these measurements because of Rowland's ideas concerning the necessary accuracy of such measurements, and the desirability of measuring all of the lines in the spectrum. Rowland at that time was not much interested in line-of-sight work, and, in fact, work of that kind was not far developed at the time, and he saw no need of an accuracy greater than was sufficient to identify solar with metallic lines, particularly as he did not consider the wave-lengths of the lines in his "New Table of Standard Wave-Lengths" to be more accurate than one-hundredth of an Ångström

unit. Also he did not favour the measurement of the very faint lines of the solar spectrum which were difficult to see, and, in fact, he disbelieved in the existence of many of them. He also disapproved of the measurement of more than one, or at the most two, standard lines at each end of a set of measurements, in order that measurements made at different times might be properly connected, saying that an accuracy greater than one-hundredth of an Ångström unit was entirely useless where the measurement of all of the solar lines were concerned, as the standard lines did not have an accuracy greater than that. Upon these points we were not in agreement at all, and as a result a sort of compromise was arrived at.

Before the measurements were undertaken, Rowland had me take a series of photographs of the solar spectrum upon commercial photographic plates which were rather coarse-grained, and as a consequence many faint and fine lines were obliterated. This he seemed to think a desirable feature, but another important consideration was that these plates were upon the same scale as the dividing-engine screw, and all of a uniform scale, whereas neither was true of the map plates. It was only when he was convinced that the uncertainties of interpretation in the case of double and multiple lines, and the difficulty of measurements in general, would require a longer time to measure the lines upon these plates than the clean, sharp lines upon the fine-grained map plates, that Rowland consented to discard them, and have the measurements made upon his remarkably fine map plates. I had also discovered a method of reducing the measurements which required very little more work than the plates made to scale.

Finally, these plates were measured with some few of the most difficult faint lines omitted, but with their existence and their approximate positions indicated. Some

of these lines have since been measured, and the measurement of the others will present little difficulty. The lack of measurements upon a greater number of good standard lines was perhaps the greatest defect of this complete table of solar lines. Of course, the standards were measured along with the other lines, but upon plates containing hundreds of lines, and in some cases two or three thousand lines, they could not all be measured at one sitting; and it was the measurement of more than one or two standards where the different sets of measurements overlapped, to which Rowland objected. These defects can be remedied, however, without much difficulty by re-measuring at one setting, not only the lines taken as standards, but other good clean lines upon these plates. It is the intention to do this as soon as possible, and also to measure the omitted faint lines.

Notwithstanding the difficulties encountered in the making of this table of the complete solar spectrum, known as "Rowland's Preliminary Table of Solar Spectrum Wave-Lengths," the wave-lengths not only of the standard lines, but also of most of the good lines in the spectrum, are probably more accurately given than the wave-lengths of the lines in the "New Table of Standard Wave-Lengths."

Later I became interested in the subject of the spectroscopic determination of the rotation period of the Sun, and especially in the question as to whether the period of rotation varied with the elevation above the Sun's surface. This investigation was undertaken largely as a result of the discovery that a line in the solar spectrum had a somewhat composite, and in a measure a complicated, structure, the different portions of shaded lines being produced by absorption at different heights in the solar atmosphere. This investigation was carried on for more than a year, and a great many photographs containing the

comparison spectra of the opposite limbs of the Sun, or of one limb and the centre of the Sun's disc, were taken. This work was mainly confined to the ultra-violet and violet portions of the spectrum between λ3700 and λ4200; but some few plates were taken from the extreme ultra-violet to the yellow and orange parts of the spectrum.

This work was done with the utmost care, and also many sets of eye-measurements were made. These eye-observations included measurements of certain lines in the yellow portion of the spectrum, near the D lines, which are due to water-vapour in the Earth's atmosphere, and have proved to be remarkably accurate, the water-vapour lines serving as an excellent check upon any possible motion of the apparatus during the observations. Also comparisons were made of a number of metallic with corresponding solar lines, using both the naked arc, and the arc under various pressures. These observations have proved to be very satisfactory, more so than similar measurements upon photographic plates. Many photographs of arc and solar spectra have also been taken, in which all necessary data for corrections have been included.

In the reduction of observations for the determination of the rotation period of the Sun, it was soon found necessary to determine, as accurately as possible, the relative wave-lengths of an entirely new set of standard lines throughout that portion of the spectrum included in the investigation. Some of the standard lines included in Rowland's table were found to be of an unsatisfactory character, and the relative wave-lengths of all of them were found not to be sufficiently accurate for my purposes. The lines which I selected (some of which were the Rowland standard lines that were of good character, or particularly useful for other reasons) were carefully chosen for their sharpness, in both solar and arc spectra, or for

their theoretical interest, as indicated by the investigations mentioned.

The plates taken for the determination of the Sun's rotation period have been carefully measured, all of the better plates in reversed direction as well as direct, and a few of the most important plates have been measured twice both ways. A considerable portion of the work of reduction has been made, but as there were over a hundred plates measured with over a hundred lines upon most of them, and considerably more than that upon a few of them, and as the lines occurred upon both a central and the two outside strips, requiring two sets of measurements, both direct and reversed, the measurements and the necessary work of reduction required a great deal of time. In addition to this, many plates having both arc and solar spectra together were measured, and also many plates having only the solar spectrum.

Besides the plates mentioned, there are a considerable number yet which it is desirable to measure, and it is important that Rowland's map plates should be gone over again, and all of the selected lines mentioned, occurring upon them, should be carefully measured in both directions. This has been done for some of the plates, but should be done for all of them; and the plates containing arc and solar spectra in the other regions of the spectrum should be measured so as to have an unbroken series of selected lines throughout the entire range of the spectrum. When this is done, the relative wave-lengths of a large number of carefully selected lines in both solar and arc spectra will have been determined with an accuracy considerably greater than that found in present tables. Moreover, the motion of ascent or descent in the Sun's atmosphere, and the pressure of the Sun's atmosphere where the absorption producing the solar lines takes place,

will have been determined for many of the lines and data obtained for determining it approximately for all of them. Much of the work of reduction has already been done for the plates measured, but, as I have had no assistance of any kind, and for the past few years the pressure of other work has prevented my finishing it, there is a great deal to be accomplished yet, but it will be done as soon as possible.

When this work is completed, there will be available a set of carefully selected solar lines whose relative wave-lengths are as accurately determined as is possible at present, and also the wave-lengths of the corresponding arc and spark-lines. The various relations between them will have been determined as well as conditions admit, and it is also expected that the intensities of the lines will have been determined upon a rational system depending upon accurate measurements or comparisons. The basis of the system of comparisons is the varying intensities of the first line in the tail of the a group due to oxygen in the Earth's atmosphere ($\lambda = 6287{\cdot}953$); and the scale of intensities used is a quantitative one easily reproduced at any time. The actual comparisons, however, are to be made by the use of a photographic scale constructed by a method which has proved satisfactory in actual use. This will require a little, but not much, experimental work, and the actual comparisons are yet to be made; but measurements already made by this method have shown it to be entirely satisfactory.

All of the work mentioned can be accomplished without much difficulty, and when done it should be based upon the best determinations of absolute wave-lengths available.

Although without doubt Michelson's determinations of absolute wave-length are more exact than any others, it would be desirable to have them repeated by other observers

with somewhat different aparatus, and it might be well for Michelson to repeat his measurements under somewhat different conditions.

It might also be desirable to make absolute determinations of wave-length with larger and better gratings than have ever been employed heretofore for that purpose. If used with a parabolic reflector and a flat mirror in such a manner that the parabolic reflector both collimates the light and brings it to a focus, the flat grating could be made to possess advantages not had by gratings with collimators and objectives, as the focus for overlapping spectra would be identical, and photographic plates could be used in the work.

The grating should be ruled so as to avoid, if possible, errors affecting measurements by careful selection of the ruling diamond and its adjustment; and, if large gratings are made for this purpose, an effort should be made to eliminate to a greater degree than heretofore the periodic and other irregularities of ruling. I believe that this can be done.

The grating method, if it can be used with better and larger gratings, and the other parts of the apparatus better than used heretofore, has many advantages not possessed by the interferometer, in that sharp lines in both arc and Sun can be used, with which better comparisons can be made than with the cadmium lines used by Michelson, which in the arc are not so satisfactory as many other lines. Also photography can be used, and absolute determinations be made in the ultra-violet as well as in the visual spectrum.

More than one large grating should be used, and measurements should be made on both sides of the normal, with each of them. Moreover, "freak" gratings should not be used, but only gratings so ruled that the spectra

are symmetrical. Such gratings can be made, and no others should be used for the purpose.

It seems to me that, although it is desirable to have more accurate determinations of relative wave-lengths than are at present available, and also desirable that they be based upon the most accurate absolute measurements which it is possible to make, it would be a serious mistake to make any change now, without having the matter more thoroughly threshed over than it has been so far; so that what is finally adopted by scientific men will not in a few years be in need of adjustment. Also it seems to me that the set of selected lines which has been mentioned might be made the basis of a set of standard wave-lengths, with the addition of other lines, principally arc or spark lines, which in various researches have proved to be useful for reference purposes. The relationships between the solar and corresponding metallic lines should all be determined as accurately as possible. Thus we should have a working set of standard lines whose wave-lengths are given for both solar and arc or spark spectra, with their various relations to each other, and their relative wave-lengths sufficiently accurate for all astrophysical work likely to be done. Furthermore, a specially selected series of some twenty-five or fifty lines scattered throughout the spectrum, which are of good character in both solar, arc and spark spectra, should have their relative wave-lengths determined with the utmost possible accuracy with one or more concave gratings, of longer focus, larger size, and better quality than heretofore used for that purpose. These gratings should be at least of thirty or forty feet radius, and be mounted in a thoroughly satisfactory manner, in a place where the work can be done without interference from street traffic or other such troubles. The wave-lengths of this comparatively small number of lines

should be thoroughly tied together by the method of coincidences. Two or three gratings having different values for the grating space should be used, the work done by photography, and the measurements made by a dividing engine with an accurate screw made for the purpose. The concave grating possesses advantages for such work not possessed by any other instrument, but the lines in the visual position of the spectrum should also be determined by the interferometer, both as a check and because it will probably give more accurate results in the visual spectrum than the grating. But dependence should not be placed upon the one form of instrument until results with both have been obtained under the best possible conditions, and then carefully compared with each other.

Finally, the wave-lengths of the few lines referred to, which can be called standards of reference, should perhaps be made to depend upon absolute measurements by interferometer methods, of the red cadmium line, or a few other lines measured in the same way.

How accurate the absolute measurements can be made, it is difficult to say; but the relative wave-lengths of the standards of reference can probably be measured to considerably less than one-thousandth of an Ångström unit. The relative wave-lengths of lines in the working table can probably be made accurate to nearly one-thousandth of an Ångström unit for the better lines, while the others should not be much more in error.

When these sets of standard lines are placed upon a satisfactory basis, the measurements of the wave-lengths of all the lines in the solar spectrum can be readily reduced to these new values and the few omitted lines added. However, some new plates should be taken in the ultra-violet spectrum $\lambda 3100$ extending as far as possible, and be carefully measured, since Rowland's and

other plates of the solar spectrum in this region are unsatisfactory. These photographs should be made on plates as little sensitive as possible to green and yellow light, and with a grating free from diffused light. The photographs should be taken at a station having a considerable altitude above sea-level.

Plates in the extreme red, as far as possible to photograph, should also be taken, or Higgs' original negatives should be measured in the same manner as Rowland's map plates have been.

Also all estimates of intensity should be reduced to the rational system referred to. The full table giving the wave-lengths, etc., of all the lines in the solar spectrum can then be published with the corrected identifications.

All of this work cannot be done in a day, and it cannot all be done by one person; but it seems to me that it would be a mistake to make any radical changes in the wave-lengths of lines until this work has been done as far as possible; and it ought not to take very long to accomplish this result, if it is gone about in the right sort of way.

In the meantime a table can be constructed and published, giving the necessary factors for reducing the wave-lengths in Rowland's published tables, to the absolute values determined by interferometer methods; but as Rowland's standards are not entirely satisfactory in some other respects, any final change from his present system should be avoided until a better table of standards can be published.

Johns Hopkins University,
Baltimore, Md., September, 1904.

PART IV.

Preparations for the Second Conference.

The task of organising the second Conference was rendered somewhat difficult owing to the great distance separating the domiciles of the three members of the Executive Committee. A good deal of correspondence had naturally to pass, before the drafted suggestions for the constitution of the Union were ready for circulation among the members of the International Committee, it being thought desirable to prepare definite proposals before communicating with the members.

On the 24th of March a letter was received from the Warden of New College, Oxford (the Rev. Canon W. A. Spooner) inviting the Conference to meet in New College. As the time was then rapidly approaching when the locality of the forthcoming meeting had to be fixed, the Executive Committee decided to accept the invitation so kindly proffered. At a later date a second invitation, dated April 20th, was received from Mr. Janssen; this the Committee were obliged to decline with regret, as the arrangements to meet at Oxford had been definitely made when the invitation reached England.

Early in May the first of the documents (No. 9), here reprinted, was circulated among the members of the Union, and this was followed by a second one (No. 10), sent out as soon as the exact date of the Conference could be fixed. These documents were accompanied by three statements (Nos. 11, 12 and 13). A wider circle of astronomers and

physicists were invited to offer their opinion on the questions to which these documents refer. Similarly a statement drawn up by Professor Kayser (reprinted in No. 14) on the standards of wave-length was circulated among physicists interested in the subject. The replies and suggestions (No. 14) called forth by these documents were circulated among those who had indicated their intention to be present at the meeting and other members of the Union. Two of the replies (No. 16 and No. 17) arrived too late to be circulated before the meeting, and are now printed for the first time. It is also thought desirable to reprint (No. 15) two letters addressed by Professor Callendar to the Editor of the *Astrophysical Journal*, referring to a defect in the older form of the Callendar sunshine recorder which is mentioned in Dr. Gilbert Walker's statement. A communication from the Committee nominated by the St. Petersburg Academy of Sciences (reprinted under No. 18) was also not received in time to be sent out before the meeting.

No. 20 is a reprint of the suggested Constitution of the Union in the form submitted to the Conference at Oxford. The resolution referring to the co-operation of the International Association of Academies, which was passed at the meeting held in St. Louis, has been considered by that Association, and their reply is given under No. 19.

No. 9.

CIRCULAR LETTER ADDRESSED TO MEMBERS OF THE UNION.

You will be aware that a Conference was held at St. Louis on the 23rd of September, 1904, at which the desirability of Co-operation in Solar Research was dis-

cussed. The Conference declared itself in favour of such co-operation, and an Executive Committee, consisting of

Professor Svante Arrhenius,
Professor George E. Hale,
Professor Arthur Schuster,

was appointed for the purpose of preparing a provisional programme.

This Executive Committee now suggests that a further meeting be held at Oxford, at the end of September or at the beginning of October of the present year. The exact date will shortly be fixed and communicated to all the societies which up the present have signified their willingness to join the Union.

The Executive Committee has drawn up some suggestions regarding the constitution of the Union, which are enclosed and which will be submitted to the meeting at Oxford for consideration and approval. In order to save time and also to test the proposed procedure for discussion of scientific questions, the Executive Committee now invite the opinion of scientific men on two questions which seem to them to be suitable for inclusion in the work of the Union.

The first of these questions relates to the measurement of Solar Radiation. The enclosed statement, drawn up by Professor Arthur Schuster, is intended to indicate the different aspects of the question on which a discussion is desirable. The reprint of a report on Radiation communicated by Professor Violle to the International Meteorological Committee is also enclosed.

The second question which is now submitted to the consideration of the Union relates to work which can be done with the Spectroheliograph. The enclosed statement, drawn up by Professor Hale, sufficiently indicates the

nature of the questions, the solution of which may be assisted by co-operation. Attention may also be drawn to the proposals of Mr. Deslandres (*Comptes Rendus*, vol. cxxxix., p. 337).[1]

At the Conference of St. Louis, a proposal was adopted to form an International Committee through the appointment of one member by each of the co-operating committees. The question of inviting additional societies to join the Union was referred to this Committee. A list of the Societies which have joined, together with their Committees, and the name of the member (as far as is known) who is to act on the International Committee, will be found below.[2] It is highly desirable that this Committee should be completed as soon as possible, and the undersigned hopes, therefore, in cases where no name is inserted in the subjoined list, to receive the nominations at an early date.

The number of international organisations having considerably increased lately, it is desirable that overlapping of the work of different organisations should be avoided as much as possible. So far as Solar Research is concerned, a committee on questions dealing with radiation and the connexion of solar and terrestrial phenomena has been appointed by the International Meteorological Committee. It will probably be found advisable to omit for the present the investigation of the relation of the sunspot cycle to meteorological phenomena from the programme of the Union; but the question of the solar constant being of fundamental importance must form from the beginning an essential portion of its work. The astronomical and meteorological aspects of solar radiation are, however, very different, and there is no reason to doubt that some arrange-

[1] See No. 24, p. 184.

[2] This list is not reprinted, as it is substantially the same as that given in the account of the Proceedings of the Oxford Conference, p. 147.

ment can be made by which the efforts of the Meteorological Committee and those of the Union on Solar Research may be united.

The Conference at St. Louis has decided to negotiate with the Academy of Sciences at Vienna in order to obtain the approval and patronage of the International Association of Academies. The harmonious working together of different International Committees would probably be best secured by bringing them all into connexion with the International Association of Academies.

The University of Manchester has made a money grant sufficient to pay for the initial expenses of printing and postage.

On behalf of the Executive Committee,

ARTHUR SCHUSTER.

The University of Manchester,
May 1, 1905.

No. 10.

CIRCULAR LETTER ADDRESSED TO MEMBERS OF THE UNION.

DEAR SIR,

Invitations for a Conference to be held at the University, Oxford, have been received from Professor Turner and the Warden of New College, who has kindly offered suitable accommodation for the meetings of the Conference.

The date of the beginning of the meeting will be September 27th, and the meeting is expected to last about three days.

The subjects to be discussed at the Conference will include the following:

1. The fixing of standards of wave-lengths.
2. The measurement of the intensity of solar radiation.
3. Work to be done with the spectroheliograph.

I shall be glad to hear of your ability to be present and to take part in the proceedings.

On behalf of the Executive Committee,

ARTHUR SCHUSTER.

No. 11.

INTENSITY OF SOLAR RADIATION.

Drawn up by ARTHUR SCHUSTER.

(Statement accompanying No. 9.)

The measurement of the intensity of solar radiation forms probably the most important problem requiring experimental investigation at the present moment. An excellent account of the present state of the question was prepared by Mr. Violle, and presented to the International Meteorological Committee at its meeting at Southport.[1] As other men of science interested in the subject may wish to express their opinion with a view to joining in common action towards the elucidation of this problem, contributions to a general discussion are now invited. For the use of members of Societies co-operating in Solar Research, I have obtained Mr. Violle's permission to reprint and circulate his report.

The subject may be viewed from different standpoints, and may roughly be subdivided as follows:

[1] Appendix XIII. of the Report published by authority of the Meteorological Council. (Eyre and Spottiswoode, London.)

1. Measurement of the total solar radiation. This would include a discussion of the relative merits of instruments which may be used in the work. Though it may not be possible at once to obtain complete uniformity in the methods of measurement, observational methods which have been proved to be unreliable should be avoided. It would therefore be of help to some who may be able to take part in the work, if those who have had practical experience in the measurement of solar radiation would give their experience as regards the principal sources of errors in the instruments they have used, and the methods they have adopted in overcoming them. It would also be important to know whether any observer is willing to carry out observations for the purpose of comparing together the action of different instruments.

2. Measurement of the distribution of energy in different parts of the spectrum. It is of importance to have accurate measurements made of the distribution of energy in the spectrum; but it would be necessary to separate for the purpose small portions of the solar spectrum which are as far as possible free from Fraunhofer lines. The measurement of the loss in radiation due to the absorption which takes place in regions covered by the Fraunhofer lines, though not directly connected with the main object of the investigation, would form an interesting addition to it.

3. Measurement of the relative values of radiation from different parts of the solar disc, both as regards total radiation and distribution of energy

in the spectrum. This would give us information as to the loss of radiation in the absorbing layers of the solar atmosphere.

4. Methods of reduction. This is probably the most difficult portion of the problem. It involves the discussion as to whether two different stations are to be used, one at high level, and one at low level, or whether we should confine ourselves to one station, observing the solar radiation with different altitudes of the sun. Both methods have to contend with their own peculiar difficulties. A preliminary discussion would probably clear our ideas, and assist those who are planning to take an active part in the observations.

The ideal method should aim at determining (1) the intensity of radiation entering the terrestrial atmosphere, (2) the diminution in our atmosphere due to dust and molecular scattering, (3) the absorption due to aqueous vapour, carbonic acid, and ozone. If some means could be devised to approach this ideal, though perhaps only in an imperfect form, many observers would probably be glad to give it a trial.

The Organising Committee would be glad to receive communications either (*a*) from members of the various Societies co-operating in the prosecution of solar research, or (*b*) from the Committees as a whole, or (*c*) from any person interested in Solar Research to whom this circular is sent. The communications may deal either with the whole of the questions indicated above or with any portion of them, and may also include other matters belonging to the subject which may not come under any of the heads mentioned.

It will also be useful if the Societies or their Committees

would indicate whether, and in what particular, it is their intention to take part in the observations.

The Organising Committee will collect the opinions received, and publish them if possible in full—at any rate, in abstract.

It is hoped that in this manner the various aspects of the subject will be fully discussed before observers finally adopt a method which may be difficult to modify afterwards.

No. 12.

Report on Radiation.[1]

By M. J. Violle.

During the four years which have elapsed since the last Meeting of the International Meteorological Committee, work on solar radiation has followed its normal course of development.

When the Committee established a permanent Committee on Radiation, they did so with the object of showing the interest which they took in this fundamental question of Meteorology, and of correlating the efforts which were being made by different organisations so as to study it under all its varying aspects.

The question is, indeed, an extremely complex one. It demands a complete study of each of the simple radiations which go to make up the total solar radiation.

Whatever be the type of apparatus employed to isolate and measure the energy of a radiation, this energy will be known (1) on its arrival at the apparatus, and (2) on its entry into our atmosphere, if we succeed in determining

[1] Reprinted from the Report of the International Meteorological Committee (Southport, 1903) and circulated with No. 9.

(1) the loss due to the apparatus employed, (2) the loss due to the passage of the radiation through the terrestrial atmosphere. Then by taking the sum of the individual energies of all the incident radiations we can obtain the energy of the total radiation which penetrates into our atmosphere.

To obtain the energy of the radiation emanating from the sun it would be necessary to know for each component radiation:

(3) The loss which it suffers in its passage between the sun and the earth.

It would also be useful to measure for each component radiation:

(4) The loss which it undergoes in crossing the complex atmosphere of the sun (comprising in this term everything outside the photosphere).

Only under these conditions would we know the amount of the energy radiated by the photosphere.

This programme, so easy to describe, cannot be easily realised.

(1) Apparatus has been constructed which allows of the accurate measurement of the energy which a particular radiation gives up to it. Further we can always evaluate the loss which it experiences in its passage through the apparatus itself with sufficient accuracy. The first part of our programme, the measurement of the energy received during one minute (the unit of time usually adopted in actinometry) by one square centimetre of surface of the earth at a given point and at a given instant, can therefore be regarded as capable of satisfactory solution.

(2) The second part presents much greater difficulties. Spectroscopic work had already given valuable information on absorption in the atmosphere, either by the help of photography or by the use of the electric thermopile, when

Mr. Langley undertook his methodical study of the spectrum (particularly of the part between 0·45 μ and 2·5 μ), with his bolometer for different thicknesses of the atmosphere at successive hours of the day and at increasing altitudes (principally at Mt. Whitney). I need not dwell on the progress which has been made in this branch of study. I will also not repeat what I said in my last report concerning the complications introduced by our atmosphere, which are due not only to the special absorption of each radiation by each of the atmospheric constituents, but also to dispersion by dust particles of every kind suspended in the atmosphere, and also by the air particles themselves. The consequences of these complications do not appear to have been fully appreciated in certain recent researches, in which it has apparently been assumed that the amount of energy received from the sun by one square centimetre of surface placed at the limit of our atmosphere in one minute, a quantity usually called the *solar constant,* could be calculated if the exact distribution of energy in the spectra obtained at the surface of the ground and at a high level observatory were determined. But even if we allow that each individual absorption conforms to Bouguer's law, is there no danger that some rays might be completely absorbed by the upper layers of the atmosphere and consequently escape detection? And is there no further danger of an appreciable and unknown portion of the radiation being deviated from its normal direction by dispersion, or even entirely reflected from our atmosphere?

(3) and (4) But granted that by taking account of the known effects of absorption and dispersion, we are able to trace a curve showing the probable variations of the true distribution of the solar energy which reaches the terrestrial atmosphere, we can, by tracing this curve for several years, obtain information as to the variations due to different

positions of the earth in space, and to changes on the surface of the sun, more particularly in the spots, the influence of which can be otherwise discerned. This immediately leads on to a study of the influence of the solar atmosphere, which will naturally be made by comparing the energies which reach us at the same instant from different parts of the solar globe. This last part of our programme is thus directly connected with the earlier ones; and though it presents difficulties of its own, it can help to throw light on obscure points in the second part.

Mr. Langley[1] proposes to continue his researches at the Smithsonian Institution, and to carry out a programme similar to that which I have described. He has already published some very important results deduced from his bolographs, which he now obtains automatically in 15 minutes, for the portion of the spectrum between $0{\cdot}45\,\mu$ and $2{\cdot}5\,\mu$; the area of these bolographs is directly proportional to the energy received by the bolometer. The coefficient of transmission of the atmosphere was measured at noon on six very fine days for certain points on the bolographs where there are no marked absorption bands; between $0{\cdot}5\,\mu$ and $2{\cdot}5\,\mu$ it is represented by six very concordant traces corresponding to different seasons and to very different thicknesses of air. The bolographs taken about noon on every calm day from February to October, 1902, show that, between $0{\cdot}76\,\mu$ and $2{\cdot}0\,\mu$ very variable amounts of energy were received from day to day, the amounts being particularly small during the summer months. The diminution which is shown at this time of the year is due to the large quantity of water-vapour then present in the atmosphere. This is shown by a comparison of the areas of the parts of the bolographs which are known

[1] Langley. The solar constant and related problems (*Astrophysical Journal*, vol. xvii., pp. 89—98, No. 2, March, 1903).

to be greatly affected by water-vapour with those of the rest of the bolographs which are not so much affected. It is interesting to note that Mr. Langley has thus divided the solar energy into two portions of very different capacity for being absorbed, a distinction which had already been drawn by Forbes.

We now come to the measurement of the solar energy given by actinometers.

The determinations made by M. Chistoni,[1] who has worked for several years and at several stations with a Violle actinometer and an Ångström pyrheliometer, call for special mention. M. Chistoni has shown that these two instruments give comparable results, and he has carried out an exhaustive research on the Ångström pyrheliometer.

M. Ångström[2] has made a further study of his compensation pyrheliometer. With the help of this instrument he has analysed the visible radiation of a Hefner element, and has shown that Wien's law represents the distribution of energy in the luminous spectrum of the Hefner lamp, and of the incandescent lamp satisfactorily.

I will not repeat what I said in my last report concerning the electric compensating pyrheliometer invented by M. Ångström. It is well known that the instrument consists

[1] Chistoni, Misure pireliometriche fatte a Sestola nell' estate del 1901 (Rendiconti della reale Accademia dei Lincei, t. XI, serie 5a, 1902, 1er semestre, pp. 77—84). Misure pireliometriche fatte sul monte Cimone nell' estate del 1901 (Ibid., pp. 479—486 et 539—541). Misure pireliometriche eseguite a Corletto nell' estate del 1898 (Ibid., t. XII, pp. 53—57). Misure pireliometriche eseguite a Sestola ed al monte Cimone nell' estate 1899 (Ibid., 1903, 1er semestre, pp. 258—263).

[2] K. Angström, Ueber absolute Bestimmungen der Wärmestrahlung mit dem elektrischen Compensations-Pyrheliometer nebst einigen Beispielen der Anwendung dieses Instrumentes (Ann. der Phys. und Chem. Neue Folge, t. LXVII, 1899, pp. 633—648). Intensité de la radiation solaire à différentes altitudes (Nova acta Reg. Soc. Sc. Upsal, 1900). Energie dans le spectre visible de l'étalon Hefner (Ibid., 1903).

essentially of two identical very thin metal bands, which are blackened on one side and provided with a thermo-electric junction on the other. These two thermo-elements are connected with a sensitive galvanoscope, which enables us to determine whether the two bands are at the same temperature. One of the bands is exposed to the radiation, while the other is protected by a screen, and an electric current of suitable strength is then passed through the screened band until the galvanoscope shows no deflection. When this is the case, we may conclude that both bands are in the same thermal state.[1] If i be the strength of the compensating current, r the resistance of the bands per unit length, b their length, and a the absorbing power of the blackened surface, then Q the intensity of the radiation received in 1 minute by 1 square centimetre, measured in calories, is given by the equation

$$Q = 60 \frac{r\, i^2}{4{\cdot}19\, b\, a} = 14{\cdot}32 \frac{r\, i^2}{b\, a}$$

As the temperatures of the two bands are equal, the losses by radiation, convection and conduction must also be identical, and require no correction. To this important advantage the instrument adds the further one that it attains its state of equilibrium in 10 seconds, as the exposed band does not require more than 10 seconds to reach a steady state. "Pour la promptitude, cet instrument laisse donc tous les autres pyrhéliomètres en arrière."

I have already stated that the comparisons made by M. Ångström between this instrument and the one of the

[1] It appears that the disparity due to the different methods of heating may be neglected. M. Kurlbaum has shown that the difference of temperature between the surface and the interior of a sheet of platinised platinum does not exceed 0·016°C. when the temperature of the sheet is 4°C. above that of its surroundings (Kurlbaum, Wiedemann's Annalen, vol. lxvii., 1899, p. 846).

older type as adopted and modified by M. Chwolsen have shown a very satisfactory agreement between the readings of the two very different instruments, an agreement in favour of both giving correct values, and I have no doubt that the systematic investigations undertaken by M. Chistoni will establish more firmly the great value of M. Ångström's compensating pyrheliometer.

Work with the instrument has been further simplified by the substitution of an accurate milliammeter (the construction of which has now been accomplished) for the electro dynamo meter formerly used to measure i accurately, and by the use of manganin instead of platinum in the construction of the bands; the change in the electrical resistance of manganin with change of temperature is almost zero, so that the small correction which had to be applied for this cause when platinum was used can be neglected.

The only remaining difficulty is one which it possesses in common with all instruments for measuring radiation, viz., the determination of the co-efficient of absorption, a. This question, which has been already studied by MM. Crova[1] and Ångström,[2] has formed the subject of an important research by M. Kurlbaum[3] who has come to the same conclusion as his predecessors, viz., that platinised platinum, thoroughly smoked, is the best absorbent at present known.

But even this is no perfect absorber; MM. Rubens and

[1] Crova, Comptes Rendus, vol. lxxi., 1875, p. 1205; Annales de Chimie et de Physique, 5th series, vol. xi., 1877, p. 443; Comptes Rendus, vol. cxxvi., 1898, p. 707 (jointly with M. Compan).

[2] K. Angström, Ofversigt af K. Vet. Akad. Förhand., 1883, p. 385; Wiedemann's Annalen, vol. xxxvi., 1893, p. 715.

[3] Kurlbaum, Aenderungen der Emission und Absorption von Platinschwarz und Russ mit zunehmender Schichtdicke (Wiedemann's Annalen, vol. lxvii., 1899, p. 846).

Nichols have found that it absorbs the intra-red rays with difficulty. Moreover, as its properties depend on the method of preparation adopted, the co-efficient of absorption for the rays considered has to be determined specially in each case.

Among other devices, Mr. Bennett[1] successfully used a sensitised paper for following the variations of the solar radiation during the solar eclipse of the 28th May, 1900.

Mr. Buchanan[2] successfully used a steam calorimeter during the same eclipse after accurately determining the loss experienced by reflexion at the mirrors which he had arranged to reflect the solar rays to the calorimeter.

The advantages of Boys' radio-micrometer, of Paschen's bolometer, and, above all, of Callendar's self-registering apparatus, appeal strongly to many scientists on account of their simplicity, but the difficulties connected with the exact evaluation of the results are almost insurmountable owing to our ignorance of the true values of certain constants.[3]

The discussion of methods of experimenting and reduction has given rise to many important papers, by studying which an accurate survey of the question can be obtained. I must confine myself to mentioning them here[4] The

[1] S. R. Bennett, Actinometry during partial solar eclipse (Royal Dublin Society Proceed., vol. ix., 17 October, 1900, pp. 365—376).

[2] J. Y. Buchanan, Solar calorimeter for eclipse observations (Cambridge Phil. Soc. Proc., vol. xi., January, 1901).

[3] F. W. Very, Solar constant (Monthly Weather Review, vol. lxxix., 1901, pp. 357—366).

[4] J. Scheiner, Strahlung und Temperatur der Sonne (Leipzig, W. Engelmann, 1899); Crova, Sur la constante solaire (Paris, Congrès International de Physique 1900); F. W. Very, Solar constant (Monthly Weather Review, vol. xxix., 1901, pp. 357 and 366); J. Y. Buchanan, Solar radiation (Nature, vol. lxiv., September 5th, 1901); W. E. Wilson, Effective temperature of sun (Roy. Soc. Proc., vol. lxix., January 4th, 1902, pp. 312—320); S. P. Langley, loc. cit.

question of the part played by the atmosphere, as it presented itself recently to the authors of these memoirs, has specially occupied the attention of M. Ångström,[1] who has attempted to determine the exact effect of the water-vapour and carbonic acid gas contained in it. M. Ekholm[2] and M. Very[3] have studied this important question with great care. For my own part, I have continued to investigate the variations in the loss due to the atmosphere up to the greatest altitudes which can be reached by "ballons-sondes."[4] M. Assmann,[5] has also collected some interesting observations on radiation carried out from balloons.

By means of the spectrobolometer, M. Very[6] has measured the radiations emitted from different points on the solar disc; among other interesting results, he has found that the atmosphere of the sun is more transparent at its edges than near the centre.

It is thus evident that progress has been made in every direction during the last four years, and the work of the

[1] K. Ångstrom, Ueber die Bedeutung des Wasserdampfes und der Kohlensäure bei der Absorption der Erdatmosphäre (Ann. d. Physik., vierte Folge, 3, 1900, pp. 720—732); Ueber die Abhängigkeit der Assorption der Gase, besonders der Kohlensäure, von der Dichte (Ibid., vol. vi., 1901, pp. 163—173); Einige Bemerkungen zur Absorption der Erdstrahlung durch die atmosphärische Kohlensäure (Kongl. Vetenskaps-Akademiens Förhandlingar, 1901, Stockholm, n°6, pp. 381—389).

[2] Nils Ekholm, Ueber Emission und Absorption der Wärme und deren Bedeutung für die Temperatur der Erdoberfläche (Meteorologische Zeitschrift, Wien., 1902, pp. 1—26).

[3] F. W. Very, Atmospheric radiation (U.S. Department of Agriculture, Weather Bureau. Washington, Government Printing Office, 1900).

[4] J. Violle, Actinométrie en ballons-sondes (Comptes Rendus de l'Académie des Sciences, passim).

[5] R. Assmann, Die Sonnenstrahlung (in wissenschaftliche Luftfahrten. herausgegeben von R. Assmann und A. Berson, Braunschweig, F. Vieweg und Sohn; 1900).

[6] F. W. Very, The absorption power of the Solar atmosphere (The Astrophysical Journal, vol. xvi., 1902, pp. 73—91).

Sub-Committee on Radiation has largely contributed to this result. By publishing a complete and impartial review of what has been done, and by drawing attention to the objects still to be achieved, it has rendered a valuable and timely service to Science.

It is now necessary to continue the work. The study of the distribution of energy in the spectrum remains the most important part of our work, and we are assured that the labours of MM. Langley and Ångström and their collaborators in solar physics will be continued.

Correlatively with this, the action of our atmosphere must be more fully understood; the losses due to absorption and diffusion are capable of more accurate determination. The method of studying atmospheric absorption introduced by Mr. Langley will probably continue to yield valuable results. The part played by diffusion, which has been established theoretically by Lord Rayleigh, and experimentally by MM. Abney, Nichols, and Ångström, will be more precisely stated. Mountain observatories, balloons, and kites will all help to extend our knowledge. Photography of different parts of the spectrum at different heights will be a useful aid in this work. If the part played by the terrestrial atmosphere is known, the loss which the total amount of energy received at noon on a clear day at the surface of the earth by one square centimetre of surface placed normally to the sun's rays has undergone from this cause can be fixed with very considerable accuracy. This total energy is the quantity which an actinometer measures for us. We, therefore, take this opportunity of expressing a desire "that actinometric observations be made regularly and continuously at all meteorological observatories."

We should thus know the amount of energy received at each instant by one square centimetre of surface at the

place of observation, and consequently the daily, monthly, and annual amounts, which are such important factors in determining climate, would also be known. From them could be deduced an approximate value of the variations of the radiation of the sun itself.

The Ångström compensating pyrheliometer is the most suitable instrument for such researches. Failing this, any of the instruments, whose trustworthiness has been established by experiment, could be used. In all cases it is of the greatest importance only to work with suitably constructed instruments which have been carefully verified and standardised. As far as possible, a continuous self-recording apparatus and also a direct-reading instrument for controlling the readings of the self-recorder should be used. It would also be highly desirable to experiment with several instruments of different types, at least at some observatories in each country, so as to ensure more efficient control and to make a more complete comparison of the different systems.

If the International Committee will support the requests which we have advanced with their high authority, Meteorology will reap great profit from the developments which will consequently take place in actinometric studies.

No. 13.

Work with the Spectroheliograph.

Drawn up by George E. Hale.

(Statement accompanying No. 9.)

Spectroheliographs are now in regular use in India, Germany, France, England, and America. Others will soon be available in Italy, Spain, and probably Mexico. A

plan of co-operation involving the use of instruments so widely distributed in longitude should permit an almost continuous record of the state of the Sun's surface to be obtained.

The moderate dispersion available in the majority of these spectroheliographs will probably cause the record to be confined in most cases to the calcium lines. The minimum daily programme for each station should include one or more photographs:

(1) Of the Sun's disc with the second slit set on H_1 or K_1 at a wave-length to be agreed upon.
(2) Of the disc with the second slit set on H_2 or K_2.
(3) Of the prominences with the second slit set on H_2 or K_2.

It is also desirable, when sufficient dispersion is available, that photographs showing hydrogen flocculi over the entire disc be taken daily.

Special studies, involving comparison of K_1 and K_2 photographs, photography of the disc with various metallic lines, comparison of forms of prominences as photographed with calcium and hydrogen lines, etc., should be undertaken whenever possible, but no plan of co-operation in such work is contemplated at present.

Suggestions regarding co-operation in work with the spectroheliograph are desired from societies, committees, observatories, or individuals interested in the progress of solar research. So far as possible, replies received before August 12th will be printed in full or in abstract, and distributed in time for consideration at the second conference on solar research, which is to be held next autumn. On that occasion a definite programme of co-operative work will be adopted.

Suggestions are particularly desired on the following points:

(1) Are you prepared to take part in this work?

(2) If so, please describe your apparatus, and state what photographs you are willing to make on each clear day.

(3) At what hours of the day can you work to the best advantage? Could you make photographs at other hours if necessary?

(4) Assuming that the photographs of the disc are to be used (apart from special investigations) mainly to determine the distribution and area of the flocculi, and the rotation period of the Sun at various latitudes, what methods of measurement and reduction do you recommend?

(5) What method of measurement and reduction do you think should be employed in the case of photographs of the prominences?

(6) Do you wish to undertake the work of measurement and reduction yourself, or would you prefer to send the negatives (or those of a duplicate series) to a central bureau for discussion?

(7) If you desire to measure and reduce your own photographs, could you arrange to have the negatives made in duplicate, one set to be sent to a central bureau?

(8) Pending the adoption of a definite programme, what photographs will be made daily at your observatory?

(9) Please add any other remarks you may wish to make regarding the proposed work.

Observations of Sun-spot Spectra.

It is of great importance, especially during the present maximum, that both visual and photographic observations

be made of the lines that are affected in the spectra of Sun-spots. In order to facilitate the preparation of plans for co-operation in this work, you are requested to send replies to the following questions:

(1) Are you prepared to co-operate with other observatories in a systematic study of the lines affected in the spectra of Sun-spots?

(2) If so, kindly describe the apparatus employed in your work, making certain to include the size of the Sun's image at the focus of the telescope, and the resolving power of the spectroscope.

(3) What region of the spectrum, and approximately how many lines are included in your present programme of observations?

(4) What method do you employ for identifying widened lines?

(5) Would you be willing to communicate your results to a central bureau for discussion with other similar observations, after having made any desired use of them yourself?

(6) If you have no objection to doing so, please state any plans you may have for further observations for spot spectra.

(7) Kindly add any suggestions you may be ready to offer, with a view of improving and systematising the observation of spot spectra. Suggestions regarding the feasibility of securing satisfactory records of the spot lines by photographic methods are specially desired.

No. 14.

Replies to the above communications, received from Members of the Union and Others.

I. Standards of Wave-length.

1. Neue Wellenlängen-Normalen (Professor H. Kayser).

1. Die Wellenlängen aller scharfen Spectrallinien,—und über 90% aller im Bogen auftretenden Linien lassen sich unter geeigneten Bedingungen scharf erhalten,—können durch Messung der Abstände von einigen benachbarten Normalen mit den heutigen besten Gittern auf wenige Tausendstel Å. genau bestimmt werden. Voraussetzung ist, dass man in allen Teilen des Spectrums genügend viele Normalen von mindestens der gleichen Genauigkeit besitzt; es scheint aber möglich, für Normalen die Fehlergrenze auf 0·001 Å. fest zu setzen.

2. Wenn auch zweifellos viele Spectrallinien, vielleicht die Mehrzahl, noch aus Componenten bestehen, welche durch das Gitter nicht getrennt werden, so kommt dieser Umstand nicht in Betracht, solange sich die relativen Intensitäten der Componenten nicht ändern, der Schwerpunkt des Complexes unverändert bleibt. Das scheint bei den Bogenlinien der Fall zu sein, so dass die Zusammengesetztheit dieser Linien keinen Einwand bildet.

3. Durch die Messungen von Michelson und Benoit über die absolute Wellenlänge der Cd-Linien ist festgestellt, dass die Werte der D-Linien, welche Rowland seinen Tabellen zu Grunde gelegt hat, nicht richtig sind. Durch die Messungen von Fabry und Pérot ist im höchsten Grade wahrscheinlich gemacht, dass auch die übrigen Messungen von Rowland relativ zu den D-Linien nicht richtig sind, sondern Fehler bis zu einigen Hundertstel Å. enthalten. Es

ist daher dringend erforderlich, ein neues System von Wellenlängen-Normalen zu schaffen.

4. Allgemein anerkannt ist, dass mit Gittern absolute Messungen von genügender Genauigkeit nicht zu erhalten sind. Nach den Beobachtungen von Kayser ist wahrscheinlich, dass die existierenden Gitter auch für relative Messungen nach der Coincidenzmethode nicht brauchbar sind, dass sie vielmehr nur für Interpolation zwischen mässig entfernten Normalen die verlangte Genauigkeit ergeben.

5. Der einzig gangbare Weg scheint somit der zu sein, dass mit einem der Interferenzapparate, wie sie von Michelson, Fabry und Pérot, Hamy construiert sind, eine genügende Anzahl von Normalen relativ zu Michelson's Cd-Linien gemessen werden. Es ist wünschenswert, dass diese Messungen von mindestens zwei verschiedenen Beobachtern vorgenommen werden. Die Normalen sollen sich in Abständen von je 50, mindestens von je 100 Å. befinden.

6. Da die Funkenlinien im allgemeinen unschärfer sind, als die Bogenlinien, da sie ferner mit der Art der Entladung stark veränderlich scheinen, vielleicht auch in Bezug auf die Wellenlänge, so scheint es zweckmässig, das weit constantere Bogenspectrum bei Atmosphärendruck zu Grunde zu legen. Wegen der zahlreichen scharfen Linien und wegen der grossen Verbreitung des Eisens als Verunreinigung, z. B. in den Kohlestäben, scheint die Benutzung des Eisens für die Normalen am zweckmässigsten.—Die Wellenlänge der Bogenlinien scheint nur mit dem äusseren Druck zu variieren, aber so wenig, dass die Aenderungen des Barometerstandes nicht in Betracht kommen; sie scheint,—entgegen Exner und Haschek— unabhängig von der Menge des Dampfes, von Stromstärke und electromotorischer Kraft. Doch wird es sich empfehlen, die

Stromstärke in bestimmte Grenzen einzuschliessen, etwa 6 bis 10 Amp.

7. Die practische Lösung der Aufgabe erfordert also:

(*a*) Wiederholung der Messungen von Fabry und Pérot für die Eisenlinien des Bogens und Ausdehnung derselben auf mehr Linien.

(*b*) Erweiterung derselben oder einer ähnlichen Methode auf den ultravioletten Teil des Spectrums.

(*c*) Benutzung des Gitters zur Interpolation, so dass auf je 5 bis 10 Å. eine Normale vorhanden ist. Für diese weiteren Normalen werden eventuell andere Elemente, z. B. Ti zu Hülfe genommen werden müssen.

8. Erst wenn diese Aufgabe erledigt ist, wird sich die weitere Frage erheben, ob man alle Wellenlängen beziehen soll auf den bisherigen Wert der D-Linien von Rowland, oder auf den der Wahrheit sehr viel näheren Wert der Cd-Linien von Michelson. Ich würde den zweiten Weg wählen, doch lässt sich auch zu Gunsten des ersteren manches anführen.

2. *Report of Committee appointed by the National Academy of Sciences (United States).*

The Committee on Solar Research of the National Academy of Sciences begs to present the following report on the subject of standard wave-lengths:

(1) The systematic errors that have been shown to exist in the wave-lengths of Rowland's tables require that a new system of standard wave-lengths be adopted as soon as possible.

(2) Displacements of solar lines, due to pressure and motion in the reversing layer, render these lines unsuitable for use as standards.

(3) The lines of iron, supplemented if necessary by the lines of titanium and other metals, produced under such conditions (probably in the electric arc) as to insure invariability of position and sufficient sharpness for the interferometer observations, should be chosen as standard lines.

(4) Since existing gratings have been shown to be incapable of giving absolute measures of sufficient precision, the interferometer should be employed to determine the wave-lengths of the primary standards, provided that further tests leave no doubt as to its suitability. Such standards should not be farther apart than 50 Ångström units, and their wave-lengths should be measured by at least two observers, working independently. Intermediate standards may be determined by interpolation with gratings.

(5) In view of the high precision and fundamental character of Michelson's determinations of the wave-lengths of the cadmium lines, the new system of wave-lengths should be based on these determinations.

3. Vorschläge für die Schaffung neuer Wellenlängen-Normalen (von Professor Dr. T. Hartmann, Potsdam).

Um die in allen Wellenlängen-bestimmungen notwendige Einheitlichkeit zu wahren, mache ich folgende Vorschläge, über welche der Congress beschliessen wolle.

(1) Ein neues System von Wellenlängen-Normalen ist erst dann anzunehmen, wenn die Wellenlängen

aller einzelnen Normallinien an wenigstens zwei Instituten unabhängig von einander gemessen und innerhalb 0·003 Å übereinstimmend gefunden worden sind. Als Normalen empfehle ich in Übereinstimmung mit Kayser die Linien des unter Atmosphärendruck zwischen zwei Eisenelektroden in Luft brennenden Bogens bei einer Stromstärke von 6 bis 10 Amp.

(2) Die absoluten Werte dieser Normalen sind nicht abhängig zu machen von den im Laufe der Zeit immer schärfer auszuführenden Vergleichungen der Wellenlänge mit dem Metersystem, sondern dieselben sind an eine noch zu bestimmende Linie, die Hauptnormale, anzuschliessen, deren Wellenlänge durch Beschluss eines späteren Congresses ein für alle mal festgestzt wird.

(3) Die Hauptnormale soll folgende Eigenschaften haben:

(*a*) Sie soll hinreichend intensitiv sein, um Messungen mit Apparaten jeder Art zu ermöglichen.

(*b*) Die Linie soll scharf und ganz symmetrisch sein, damit die gemessene Wellenlänge nicht von der Intensität oder von der Belichtungsdauer abhängig wird.

(*c*) Die Bedingungen für die Erzeugung der Linie müssen klar und stets reproduzierbar sein.

(*d*) Die Linie soll an einer sowohl für optische, als auch für photographische Messungen leicht zugänglichen Stelle der Spektrums, also etwa zwischen den Wellenlängen λ5000 und λ6000 liegen.

(4) An einer früheren Stelle[1] hatte ich als solche Hauptnormale die rote Cadmium linie λ6439 vorgeschlagen, und diesem Vorschlage schlossen sich später[2] auch Pérot und Fabry an. Da diese Linie jedoch die Bedingung (*d*) nicht erfüllt, so ist die Wahl einer anderen Hauptnormale zu empfehlen. Es ist erwünscht, dass Besitzer von Interferenzspektroskopen die Struktur einer Anzahl geeigneter Linien untersuchen, so dass dem nächsten Congress Vorschläge für die definitive Wahl der Hauptnormalen vorgelegt werden können.

(5) Welchen Wert man für die Wellenlänge der Hauptnormalen annehmen will, ist ziemlich belanglos. Es kommen zwei Vorschläge in Betracht.

(*a*) Man kann die Wellenlänge so wählen, dass das neue Wellenlängen-system möglichst genau mit dem Metersystem übereinstimmt. Der Vorteil dieses Verfahrens ist sehr gering, da ja eine völlige Übereinstimmung beider Systeme doch nicht zu erreichen und für alle Spektraluntersuchungen der absolute Wert der Wellenlängen immerhalb gewisser Grenzen ohne Bedeutung ist.

(*b*) Man kann die Wellenlänge so wählen, dass das neue System möglichst nahe mit dem Rowlandsehen Wellenlängensystem zusammenfällt. Dieses Verfahren bietet für die Einheitlichkeit aller Wellenlängen Angaben ausserordentlich grosse Vorteile und ich empfehle daher seine Annahme dem Congress.

[1] *Astrophys. Journ*, vol. xx., p. 46, July, 1904.

[2] *Astrophy. Journ.*, vol. xx., p. 326, December, 1904.

(6) Bis zur Einführung der neuen Wellenlängen-Normalen sind alle genauen Spektralmessungen an ein möglichst stetiges System von Normallinien auszuschliessen, d.h. an ein System, dessen einzelne Wellenlängen frei von groben zufälligen Fehlern einzelner Linien sind. Daher sind als Vergleichslinien zu benutzen (*a*) von λ2327 bis λ4495: die Kayserschen Normalen aus dem Bogenspektrum des Eisens und (*b*) von λ4737 bis λ6495: die früher[1] von mir mitgeteilten, auf das Rowland system übertragenen Wellenlängen der Eisen-Bogenlinien nach den Messungen von Pérot und Fabry.

Die Anwendung der aus Rowlands Sonnenspektrum entnommenen Wellenlängen für die Bogenlinien des Eisens ist *durchaus* zu verwerfen.

4. *Statement of Professor Ames on behalf of the American Physical Society.*

In regard to the first question, that of standards of wave-lengths, we would like to call attention to the fact that at the Johns Hopkins University a research is already in progress to redetermine as nearly as possible Rowland's standard wave-lengths by the use of a Fabry and Pérot Interferometer. This work has been carried on by Professor Ames and Dr. Barnes, and considerable progress has already been made. It is their intention to apply a photographic process which can be used in the ultra-violet as well as in the visible spectrum. The measurements of Michelson of the absolute wave-lengths of cadmium will serve as a basis. The lines which will be first measured

[1] Zeitschrift für wissenschaftliche Photographie, vol. ii., p. 171, 1904; *Astrophys. Journ.*, vol. xx., p. 47, 1904.

are the arc lines and the solar lines used in Rowland's tables, which are of sufficient intensity for the purpose.

For purposes of interpolation many measurements of the spectra are necessary. All the results both in the arc and spark obtained by good gratings are at the disposal of the Committee, and will be used as soon as the standards themselves are fixed. Special attention in this respect has been given the spectra of iron and of titanium.

5. Statement by Sir David Gill.

I have received a copy of Professor Kayser's note on new wave-lengths of normal lines.

I agree with every word of it.

If we are to start a new system of wave-lengths, which is most necessary, for any sake let it be fundamentally the best that can be arrived at, and not a patching up of Rowland's system, which is unquestionably very shaky.

We may, I think, accept the wave-lengths of the cadmium lines determined by Michelson and Benoit as probably sufficiently exact for an absolute basis—we certainly are not likely to get a better determination for a long time.

The wave-lengths of certain iron and titanium lines should then be selected as Secondary Standards and be independently determined by interferential methods by two observers, each of whom shall, with his own apparatus and methods, also determine the wave-lengths of the cadmium lines. This will afford means of reducing all the secondary determinations to the same standard.

Tertiary Standards could thereby be determined from these by interpolation with the aid of gratings, but without the method of coincidence of spectra of different orders.

6. Statement by Professor C. G. Abbot.

(*a*) The present system of wave-lengths, based principally

on the measurements of Rowland, appears to be affected with both absolute and relative errors which, while minute, are yet of serious inconvenience in many spectroscopic researches.

(*b*) While the absolute error of the system as a whole is of small moment, the correction of the relative errors, which is essential, will be so far reaching that the slight additional step of reducing the whole corrected system to accord with the best determinations of absolute wave-lengths ought to be made at the same time.

(*c*) Expert consideration should be given to the possibility that hitherto unrecognised errors may affect interferometer determinations of wave-lengths, and to the precautions necessary to be observed to obtain definite determinations of wave-lengths under standard conditions, which may at any time be duplicated. I would recommend the appointment of a Special Committee to determine these matters.

(*d*) If nothing to the contrary appears from such proposed expert consideration, the new system of standards might well consist of not less than 80 well-distributed lines of the iron and other arcs under stated considerations of pressure and electrical circuit, and all of these should be connected by interferometer determinations with the cadmium line determinations of Michelson and Benoit.

(*e*) Interpolation between such standards may be made with the grating.

7. Professor G. E. Hale writes :—

The report of the Committee on Solar Research of the National Academy of Sciences corresponds with my views on the subject of new standards of wave-lengths. I believe that a Standing Committee, composed principally of spec-

troscopists, who have given special attention to the subject, should be charged with the duty of investigating the best means of procedure and the subsequent work of securing the necessary measures. An important object of this Committee should also be the preparation of a table of Solar wave-lengths, based on the new standards.

8. Short Summary of the above Views, drawn up by Professor Arthur Schuster.

The opinions expressed in the above statements are in agreement on the following points:—

1. There should be one primary standard to which all other wave-lengths are to be referred.
2. There should be secondary standards, the distances between which should, if possible be 50 and never more than 100 Ångström units apart. These secondary standards should be referred to the primary standard by means of some interferometer such as that of Fabry and Pérot. The secondary standards should be lines obtained by an electric arc of from 6 to 10 Amp. Before the final adoption of the wave-lengths of the secondary standards in terms of the primary one, it would be necessary to obtain at least two independent determinations which are in substantial agreement.
3. There should be tertiary standards at distances of from 5 to 10 Å. These tertiary standards are to be determined by interpolation with the help of gratings.

A difference of opinion seems to exist as to the choice of the primary standard. According to Professor Hartmann it should be chosen so that the resulting wave-lengths are

as far as possible in agreement with Rowland's scale. But the balance of opinion seems to incline towards the selection of the red cadmium line, the wave-length of which was determined by Michelson and Benoit. The final decision of this matter need not be taken until the secondary standards have been fixed, and may be deferred therefore to a subsequent meeting of the Union. Some new light may in the meantime be thrown on the suitability of choice by further investigation; but a discussion of the question at the present moment would, in my opinion, be useful.

Sir David Gill and Professor Hale suggest the appointment of a Committee to whom the choice of the primary and secondary standards might be left. The necessity of rapid action in the matter will be apparent to all.

II. Solar Radiation.

1. Professor Abbot's Statement.

(*a*) Measurement of Total Solar Radiation.

There is now no well-known system of absolute pyrheliometry. The indications of instruments hitherto employed for measuring solar radiation cannot be checked by the observation of known quantities of heat, so that observers are altogether dependent on the soundness of the theory of the instruments they employ. All the well-known types of actinometers and pyrheliometers are subject to the objection that of the radiation absorbed at the receiving surface, a portion is inevitably lost by air convection and by radiation from that surface. This escaping portion is uncertain in amount, and does not produce any measurable effect upon the recording apparatus.

In Pouillet's pyrheliometer and Tyndall's modification of it, in Violle's globe actinometer, Crova's alcohol actinometer and others, the rays are received on a blackened sur-

face behind which is situated a thermometer. It is apparent that only a portion of the heat absorbed at the surface is conducted to the rear to raise the temperature of the thermometer, and that the remainder of the heat due to the rays absorbed is lost by convection and radiation from the absorbing surface, and cannot be estimated either from the rise of temperature during the exposure, or from the cooling correction.

A similar objection holds regarding Ångström's compensation pyrheliometer, for in this instrument the measurements of the thermo-couples indicate merely that the rear surfaces of the two strips are of equal temperature, while the cooling of the strip which is warmed by the sun rays, and which is warmest at the front, must be greater than that of the electrically heated strip, to which the heat is supplied from the interior. Accordingly, the heat supplied electrically is less than the heat absorbed from the sun.

In all these instances the loss is aggravated by the circumstance that one and, sometimes, two coats of porous blackening material is applied to the receiving surface of the instrument, so that the conductivity is diminished and the convection and radiation augmented. It might be better to employ a clean surface rather than a blackened one, for though the loss by reflection would then be greater, yet this could perhaps be determined and corrected for, while the unknown errors of convection and radiation would be diminished.

It has been suggested to me by Mr. L. R. Ingersoll that if each of the thermo-couples of the Ångström pyrheliometer were placed between two blackened strips, and if good reflecting surfaces were introduced in such a way as to cause the sun rays to fall on the back as well as the front of these double strips, the source of error above mentioned

would perhaps be avoided. The introduction of reflecting surfaces would be objectionable but not perhaps fatal to the merits of this plan.

I have myself endeavoured to avoid the losses by reflection, convection, and radiation at the receiving surface by designing a new form of pyrheliometer. An instrument of this new kind has been constructed by Mr. Langley's direction, and is now on Mount Wilson, California, as a part of the apparatus of the Smithsonian Expedition.

The receiving surface is a hollow blackened cylinder with its rear end, upon which the rays strike, of conical form. A series of diaphragms within this chamber is introduced to diminish convection, and a further series of diaphragms for the same purpose is provided within a separate tube, through which the rays pass before entering the chamber. The last of these outer diaphragms close to the entrance of the chamber, being of a measured aperture smaller than any of the others, determines the area of the cross-section of the beam. An outer jacket is provided enclosing the hollow chamber, and a measured current of water is introduced at the front of this jacket, and after circulating around and bathing the wall of the chamber passes out at the rear just behind the conical absorbing surface. Four platinum wires are provided and appropriately joined to form a Wheatstone's bridge, and are further provided with slide wires for adjusting the bridge and for measuring the sensitiveness of the apparatus. Two wires are bathed by the liquid before it bathes the walls of the chamber and two just as it leaves it, so that the rise of temperature of the liquid due to the absorption of the solar rays is thus determined. It is obviously of no consequence whether this absorption takes place altogether on the conical rear wall of the chamber, or after reflection, convection, and radiation it takes place nearer the entrance,

for in either case it warms the liquid between the two pairs of platinum wires.

The principal danger of loss is from the conduction and convection of heat through the liquid and convection from the outer wall of the jacket by the air. To guard against this a second jacket, separated from the first by an air space, guards the rear or warmer end of the inner jacket, and to this outer one the liquid is first admitted after passing the first pair of wires and before gaining the front end of the inner jacket. In the second place the loss of heat towards the outside may be diminished as much as necessary by increasing the rate of flow of the liquid.

A main advantage of the instrument consists in the possibility of checking the accuracy of its readings. For a coil of wire is provided at the rear within the hollow chamber, and through this coil a known current of electricity may be passed, so that a known heating effect is produced in it. This heat may then be measured as if it were due to the absorption of sun rays. Such tests have shown that the heat thus introduced can be measured, through the rise of temperature of the water, to within about one per cent. A still closer measurement of the solar heating is to be expected, for the sun rays are absorbed directly upon the wall of the chamber for the most part, whereas the heating of the coil is first chiefly communicated to the air and thence by convection to the walls of the chamber.

This pyrheliometer has been provided with adjuncts for measuring and automatically recording the flow of the liquid, the passage of time, and the deflection and zero of the galvanometer, and the pyrheliometer proper is mounted equatorially, and driven by a clock. It is thus intended to produce a continuous automatic absolute measurement of the solar radiation.

A good secondary pyrheliometer may be made very cheaply, using Tyndall's modification of Pouillet's pyrheliometer. As a still further improvement, copper may advantageously replace iron as a containing vessel for the mercury. The pyrheliometer should be surrounded by a poorly conducting chamber, to which the rays are admitted through a long diaphragmed tube. Several instruments of this kind are in use at the Smithsonian Astrophysical Observatory. The results of numerous comparisons have convinced me that the measurements of the mercury pyrheliometer, the Crova alcohol actinometer, and the Ångström compensation pyrheliometer are closely proportional.

Mr. R. E. Fowle has lately shown that measurements of total radiation continued from high to low sun may be graphically extrapolated in accordance with Bouguer's exponential formula to yield a value of the total radiation outside the atmosphere, and that the immediate result thus obtained is, for Washington observations, always between 12 and 15 per cent. lower than the result obtained simultaneously from observations of the spectro-bolometer on homogeneous rays. It is remarkable that the difference is so uniform and varies so little with the character of the atmosphere transmission. It is not to be supposed that the same correction (averaging 14 per cent.) is applicable to all stations, but doubtless a similar treatment of pyrheliometric results would, for any station, yield values proportional to the solar constant from which evidence of the variability of solar radiation might be inferred.

(*b*) Measurement of the distribution of energy in different parts of the spectrum.

As shown by Langley it is essential to know the transmission of each spectral ray in our atmosphere in order to determine the solar constant. In practice only an ap-

proximation to this complete knowledge is possible, so that an element of uncertainty must remain as to whether the error from this cause is not considerable. With the linear bolometer as applied for solar constant work at the Smithsonian Institution, the sum of the angular widths of bolometer and slit is about 40 secs. of arc, which corresponds to about 9 Ångström units on the average scale of dispersion of the prism between A and K of the spectrum. Obviously many of the finer spectral lines are thus confused in producing a general effect upon the bolometer. Only one check upon the accuracy of the results of spectro-bolometric determination of the solar constant by the method of high and low sun measurements seems available. This is to make simultaneous determinations of the solar constant at different altitudes, reducing each set of observations independently. If no appreciable discrepancy appears as a function of the altitude, then the error from want of homogeneity of the spectral rays, and other unavoidable uncertainties attending solar constant measurements by high and low sun observations at a single station, will be shown to be negligible.

For the purpose just mentioned spectro-bolometric measurements are now being made simultaneously at Washington and at Mount Wilson by the Smithsonian Astrophysical Observatory. At each station bolographic energy curves extending from wave-length $0{\cdot}38\mu$ to wave-length $2{\cdot}8\mu$ are made about twice an hour on many good days. These curves represent the distribution of the spectrum continuously between the limiting wave-lengths just stated, including regions filled with Fraunhofer lines as well as those but sparsely occupied by them. In measuring the ordinates at selected points for the purpose of determining the general transmission of the atmosphere, preference is given to points where the Fraunhofer lines

are not very abundant. I am not aware, however, that the terrestrial transmission coefficients would vary appreciably as between the centre of a solar line and a point in the continuous spectrum immediately adjacent. Determinations of the transmission coefficients in the principal water vapor bands of the infra-red spectrum have been made, and it appears that practically similar results are arrived at as to the original intensity of the solar rays of these wave-lengths outside the atmosphere, whether these selected transmission coefficients in the bands themselves are employed, or a smooth curve is drawn over the absorption bands and the extrapolation is made by the aid of the general transmission coefficients determined for neighbouring wave-lengths. In practice, however, the latter method is adopted.

(*c*) Measurements of the relative values of radiation from different parts of the solar disc.

The distribution of intensity along a diameter or chord of the solar disc is most accurately determined by allowing a large image of the sun to drift by the earth's diurnal motion across a slit which limits the area investigated at any instant, and behind which is a linear bolometer or other radiation measurer. More detailed and interesting knowledge may be gathered by interposing a spectroscope between the bolometer and the slit, and setting this spectroscope successively for selected wave-lengths. The advantage of allowing the image to drift instead of turning the telescope or of fixing the image on a selected region of the sun and turning the spectroscope so as to produce a continuous energy curve, lies in the fact that all such defects of the optical train as inequality of reflecting power and defective following are wholly without influence on the relative observed intensities of the radiations from different

regions when these regions are brought to measurement by the process of drifting.

Within the past 50 years spectro-bolographic measurements of this character, taken at about 12 points in the spectrum from $0{\cdot}4\mu$ to $2{\cdot}5\mu$, have been made on all favourable occasions at the Smithsonian Astrophysical Observatory by Mr. Langley's direction. In many instances sunspot regions have been specially selected, but in general the drift has been directly across the disc on a diameter.

(*d*) Methods of reduction.

In the atmosphere of the earth and the envelope of the sun it may be assumed that for strata so thin as to be essentially homogeneous in quality and density the transmission for each wave-length follows the exponential formula of Lambert and Bouguer. It may be shown that for such a medium composed of parallel plane strata, however thin, each of which is homogeneous in quality and density, but differing in quality or density, or both, from its neighbours, the transmission of the whole system for any given wave-length may also be represented by a similar exponential formula, provided the direct reflection and refraction due to the variation of optical density may be neglected. In such a formula the transmission coefficient for the whole system of layers is the product of the transmission coefficients of all the separate layers.

The atmosphere of the earth, so far as it is dense enough to exercise appreciable absorption, may be regarded as so thin compared with the earth's radius that for zenith distances less than 65° the thickness of each of its concentric layers, and of all of them combined, is proportional to the secant of the zenith distance. Nor is the error of thickness appreciable when Laplace's refraction air mass formula is adopted for zenith distances between 65° and

75°. Beyond this zenith distance it is unnecessary to go.

If then we assume that the disturbances due to air currents, mountains, and the like are neglible, and that we may regard the earth's atmosphere as essentially a system of innumerable concentric layers of neglible regular reflection and refraction, whose quality and density varies from layer to layer but not within any single layer, then writing E_0 and E for the energy of any wave-length outside the atmosphere and at the surface of the earth respectively, a the transmission for vertical incidence, and m the air mass, we have:

$$E = E_0 a^m.$$

whence

$$\log E = m \log a + \log E_0.$$

In spectro-bolometric work we assume the deflection of the galvanometer d to be proportional to E after correcting for the losses of selective reflection and absorption within the optical train. Hence we write:

$$E = kd$$

and

$$\log d = m \log a + \log E_0 - \log k.$$

This is the equation of a straight line if E_0 and k are constant during the observations. Accordingly, the truth of the assumptions made regarding the nature of the transmission of the earth's atmosphere will tend to be verified if the observations on homogeneous rays made from noon towards night of a good cloudless day can be best represented by a straight line plot whose ordinates are proportional to logarithms of galvanometer deflections, and whose abscissæ are air masses.

In fact, the observations of the best days at Washington are best represented, by such plots, and indeed the error of representation is on such occasions within the probable error of the observations. Other checks of value are found in the facts that the employment of this method of extra-

polation yields closely agreeing values of the solar constant for days of very different atmospheric transparency and for days of very different solar declination. Another valuable check would be found if solar constant measurements by this method made at very different altitudes should yield accordant results independent of the altitude. But if a progressive increase of the solar constant with the altitude of observation should appear, then a correction factor depending on the altitude of the observing station could intelligently be introduced.

In the case of the sun, its absorbing envelope is so far unknown as regards density, temperature, and thickness that nothing quantitative seems possible to determine as to its transmission. The observations made on the solar image above referred to can, however, be closely represented by making the following arbitrary assumptions:— First, that the solar absorbing envelope is homogeneous in quality and density. Second, that its radiation is negligible. Third, that its thickness is 21 per cent. of the solar radius. On these assumptions the transmission coefficients may be determined from straight line logarithmic plots, just as for the earth's atmosphere. Inasmuch, however, as the assumptions are clearly unwarrantable in fact, they can only be defended as yielding a convenient scale of abscissæ for graphically representing the results of observation in such form that the observations of one day may be easily compared with those of another.

(*e*) Variability of the Sun.

Three kinds of evidence indicate a substantial variability of the intensity of solar radiation outside our atmosphere. First, measurements of the solar constant have indicated a variation of 10 per cent. or more at several times within the last two years. Second, measurements of the absorption of the solar envelope have indicated a smaller absorp-

tion when the solar constant is high, and *vice versa*. Third, temperature departures from the normal at the principal meteorological stations of the north temperate zone have indicated lower temperatures generally over this region soon after a fall of the solar constant and *vice versa*.

It is important to verify these conclusions, to obtain if possible independent evidence in their favour, as for instance from the comparative brightness of the planets and the stars, and to make radiation measurements so often and so accurately that the variations may be more exactly known.

2. *Suggestions for Measurement of Intensity of Solar Radiation by Dr. Gilbert Walker.*

1. *Measurement of the total solar radiation.*

Solar radiation thermometers have been in use for 20 years or more at a number of stations in India, but the observations are of doubtful value. The readings of the scale appear gradually to decrease, and it may be inferred that the absorbing power of the black diminishes with time. It may be that a curve showing the rate of decrease could be constructed and that from it the true values of the readings of the thermometers could be determined. It is intended to compare these thermometers with Ångström's pyrheliometer to see if such a curve can be obtained and some use made of the past observations.

A Callendar sunshine recorder has been in use for some time at Calcutta but the results have been very unsatisfactory and unreliable. The chamber containing the resistance is not hermetically sealed, and consequently water vapour condenses inside to such an extent that at times drops of water fall on the wires. The lamp black has also peeled off the platinum in some places and hence the con-

stants of the instrument have changed.[1] A new set of resistances with black enamel instead of lamp black and placed in an exhausted chamber is expected shortly, and it is hoped that systematic observations may be made with it. The instrument will also be compared with the Ångström's pyrheliometer either at Simla or Calcutta and its constants determined.

It is proposed to initiate observations with Ångström's electric compensating pyrheliometer, and to establish regular measurements of the total solar radiation at two places. For this purpose it would seem natural to select Simla (Latitude 31° 6′ N., Longitude 77° 12′ E., height above sea level 7,200 feet) and, as the second place, the nearest Observatory where the necessary superintendence is available; probably the most suitable second place will be Allahabad (Latitude 25° 26′ N., Longitude 81° 52′ E., height above sea level 300 feet). It is proposed to make a comparison of the new Callendar sunshine recorder with an Ångström's pyrheliometer.

2 and 3. It does not at present seem possible to make any observations under these heads.

4. *Methods of Reduction.*

Information as to the absorption of the atmosphere will be obtained from observations made at different altitudes of the sun at both Simla and Allahabad, and there will also be the continuous record of the Callendar sunshine recorder.

During the monsoon period air is saturated with moisture and during the hot weather the air is filled with dust; there are therefore very good opportunities of making observations on the absorption due to water vapour and also on the diminution of intensity due to scattering by dust.

[1] With respect to this point see Prof. Callendar's letters to the *Astrophysical Journal*, reprinted p. 129, *infra*.

It will perhaps be possible to make occasional observations with Ångström's pyrheliometer on the Chur Peak, whose altitude is 12,000 feet, but the difficulties of access are extremely great and it is not likely that opportunities for such work will often occur.

3. Statement by Professor Ricco.

Méthodes de réductions. Il parait préférable d'avoir deux stations à des altitudes differentes. En effet le Dr. A. Bemporad, assistant a l'Observatoire de Catane, a demontré qu'en faisant une série de combinaison des observations correspondentes, simultanées, deux à deux, des deux stations pour des distances zénitales égales d'un astre, on peut avoir autant de déterminations absolues de la valeur du coefficient de transmission, qu'il y a de distances zénitales des observations accouplées. Naturellement il faut pour cela déterminer exactement les masses d'air comprises entres les deux stations pour toutes les distances zenitales, ce qui actuellement peut se faire avec beaucoup de precision, en se fondant sur la connaissance exacte qu'on a à present de la constitution et des conditions météorologiques de l'atmosphère jusqu'à de très grandes hauteurs, de la lois de refraction etc. Le Dr. Bemporad a calculé des tables qui donnent ces masses de 0m. jusqu' à 5,000m., pour toutes les distances zenitales de 0° à 89°. Le Dr. Bemporad en a fait l'application aux observations photometriques executées en 1894 par MM. les Professeurs Muller et Kempf simultanement à l'Observatoire de Catane et à l'Observatoire sur l'Etna, avec une différence de hauteur de 2,900 m. Il a trouvé que le coefficient p de transmission de l'air croit avec la distance zénitale, étant pour toute atmosphère $p=0{\cdot}231$ pour $z=0°$, et $p=0{\cdot}553$ pour $z=82°$, environ du simple au double. Ainsi il est demontré :—

(*a*) Que (comme M. le Professeur Langley l'a affirmé depuis 20 ans) le coefficient de transmission est beaucoup plus petit que 0·80 que l'on admettait auparavant.

(*b*) Que la formule de Bouguer $Q_1 = Q\ p^{-m}$ qui suppose p constante pour toutes les valeurs de λ n'est pas admissible.

(*c*) Qu'au contraire est vrai l'absorption sélective de l'air, que M. Langley avait preconsisée.

Le Dr. Bemporad a appliqué ses methodes aussi aux observations actinometriques de Ångström à Teneriffe, et il a trouvé : —

A. Est confirmée l'absorption décidément sélective de l'atmosphère, même pour les radiations calorifiques.

B. En considerant des altitudes differentes, de 0 m. à 3,500 m., pour les rayons qui ont traversé des epaisseurs egales d'air, le coefficient de transmission est proportionel à la 4 me puissance de la densité de l'air.

Cette relation presente une analogie frappante avec la loi Thermodynamique de Stephen et Lord Rayleigh. Ces analogies pourraient ne pas être accidentelles. Le résultats des études du Dr. Bemporad sont en faveur de la methode de determination de l'absorption atmospherique avec deux stations de hauteur differentes.

4. Professor Hale writes : —

Mr. Abbot's important communication on this subject will undoubtedly receive careful consideration. As the conditions on Mount Wilson have proved to be very favourable for this work, arrangements will be made to continue

for several years the observations now being made by the Smithsonian Expedition. Similar work should also be carried on regularly at some other elevated station where the weather is clear for many weeks in succession, such as the Observatory at Kodaikanal, India. Mr. Abbot believes that good determinations of the solar constant can also be obtained at a low-level station, but unless situated in a favourable climate, such a station can never afford the unbroken succession of clear days necessary for the satisfactory study of possible variations of the solar radiation. It is not enough to detect the existence of such variations, though to do so must constitute a most important advance; it is of the highest importance to determine whether the maximum is reached suddenly or gradually and whether other solar phenomena undergo a corresponding change.

It is to be hoped that the study of the solar absorption will be taken up by several Observatories. Variations of the absorption will be of the greatest interest, both in connexion with the solar constant observations and the results of work with the spectroheliograph.

5. Mr. Filipe Valle writes:—

I have already ordered some time ago a pyrheliometer according to Professor Scheiner's design, and I will try to make some expeditions to the higher mountains of this country, for the determination of the solar constant and its variations, as often as possible. We will take also here actinometrical observations with a self-registering actinometer, and besides that we have already in working order a small magnetical observatory beyond the action of the currents of the electrical railways. You may see that we are able to co-operate in the solar research and connected branches; we want only to receive the principal instrument, the spectroheliograph, to begin preliminary work,

though on not so large a scale as that soon to be developed in the Willson Observatory, South Kensington, and so on.

6. Professor E. B. Frost (Yerkes Observatory) writes:—

I should say in this connexion that I am beginning a series of observations of the intensity of radiation from sun spots and the different parts of the surface of the sun in continuation of the work along that line which I did in 1891.

With our present staff, it will not be possible at this present time to take up other investigations in the intensity of solar radiation.

7. Statement by Professor Arthur Schuster.

The great interest shown in the determination of the solar emission of energy renders it certain that our knowledge of it will be materially increased within the next few years. It is, I think, generally recognised that the measurements should be made as far as possible with the same instrument, and Ångström's compensation pyrheliometer seems to fulfil all necessary requirements in this respect. The only objection raised against the accuracy of its direct indications is that mentioned in Mr. Abbott's communication. The heat supplied to one strip is applied internally and on the other which is exposed to the sun it is absorbed at the front surface, which therefore becomes hotter than the corresponding surface of the electrically heated strip. In Mr. Violle's communication a reference will be found to Kurlbaum's investigation, according to which the error introduced is very small. At any rate it is an error which is capable of being determined and corrected for. It would therefore be a mistake, in my opinion, to try to displace Ångström's instrument by another which though perhaps more accurate would be more difficult and

cumbersome in practical use. It would, however, be highly desirable if Mr. Abbot were to compare directly the indications of the instrument he describes with those of other pyrheliometers. Such work, if done at one or two well-equipped stations, would materially add to our knowledge of the comparative value of different instruments and might lead to the detection of small sources of error.

It is well known, however, that the mere recording of the total energy of solar radiation does not give us information sufficient to eliminate the effects of terrestrial absorption; but that a complete determination of the solar constant involves measurements in different parts of the spectrum. It is not, however, in my opinion, necessary to have recourse to the extreme refinement of working with homogeneous radiations. We may take it that the variable portion of solar radiation is due to absorption of water vapour, carbon dioxide, ozone, and to the effect of dust. The latter showing itself as a continuous absorption is difficult to deal with, and for this reason it seems to me essential to conduct our principal measurements at high-level stations where the effects of dust are small. In a dust-free atmosphere there must be a definite ratio between the energy in a portion of the spectrum which is free from the absorption effects of the three above gases and the total energy as affected by the absorption of oxygen and nitrogen but not by the variable constituents. When this ratio has been determined once for all, we may continue to work with the selected portion of the spectrum. I believe Professor Ångström has devised some means of filtering the total radiation through coloured media, so as to enable his instrument to be used for the determination of solar energy as unaffected by the absorption of the variable gases. The complete elimination of the effects of nitrogen and oxygen can only be carried out after the law of scattering and

absorption of these gases as dependent on wave-length has been investigated. This part of the work need only be undertaken on one or two well-equipped observatories.

III. Spectroheliograph.

1. Statement by Professor Hale.

For several reasons it is important that the sun should be photographed as often as possible with the spectroheliograph at many stations widely separated in longitude:

(1) The calcium flocculi are much greater in area than the sun-spots, and continue to appear at sun-spot minima, when spots are often absent from the disc for weeks together. They may therefore serve as a better index to the true state of the sun's activity, and their variation in distribution and in area may prove of significance when studied in connection with such phenomena as the variation of terrestrial magnetism.

(2) The spectroheliograph furnishes the readiest means of detecting eruptive phenomena on the sun's disc. A practically continuous record of the flocculi, made through the co-operative use of spectroheliographs widely distributed in longitude, would give a list of such eruptions for comparison with the record of magnetic storms and for study in connection with possible changes in the solar absorption and the intensity of the solar radiation.

(3) In investigations on the nature of sun-spots it is important to learn whether the appearance of flocculi is the first indication of the disturbance

which ultimately results in a spot, and whether the flocculi tend to precede or follow the spot in their motion across the disc.

(4) Since the law of the solar rotation is not known to be the same for different elements at different levels above the photosphere, the spectroheliograph furnishes an excellent means of answering this question, and of supplying a check on the results of spectroscopic measurement. The study of the rotation is also facilitated by the wide distribution of the flocculi in latitude and by their persistence throughout sun-spot minima.

(5) A photographic record of the chromosphere and prominences at the limb will serve as a useful supplement to the results of visual observations and supply the means of detecting differences in the distribution and relative intensity of the calcium and hydrogen in these objects and of determining their relationship to the flocculi.

(6) Photographs of the disc, made simultaneously or in rapid succession with the lines of calcium and hydrogen, will permit valuable comparative studies to be made of the form, area, distribution, brightness, and motions of the calcium and hydrogen flocculi. It is not unlikely that such hydrogen photographs will prove to be the most sensitive means of detecting small eruptions, in view of the fact that the quiescent hydrogen flocculi are dark, while the eruptions are bright.

In most of these investigations the advantages of co-operation among many stations well distributed in longitude will be apparent. The observing programme of each station should, so far as this is feasible, be prepared with regard to the general end in view, *i.e.*, to secure such a

distribution of the photographs in time as would most closely approximate a continuous record of the state of the sun's surface. It is unnecessary to say that whenever the spectroheliograph record can be supplemented by spectroscopic observations this should be done. Photographs of the *H* and *K* lines at many points on the disc are particularly desirable, both for the purpose of determining the direction and the velocity of the ascending or descending currents of calcium vapour, and for the better interpretation of the spectroheliograph results.

In the foregoing enumeration of some of the uses of the spectroheliograph, I have included only such classes of work as are within the reach of existing instruments. It is hardly necessary to say that a much wider field of research lies open to those who will prepare the necessary apparatus.

Without going into details here, I may say that the apparatus required for the most promising new applications of the spectroheliograph, such as the use of the dark lines of various elements, the photography of sun-spots with widened lines, etc., can be fitted up at very moderate expense, in large part from instruments and optical parts already available in many observatories. The impression should not prevail that very special and expensive apparatus is required for such work. The spectroheliograph temporarily in use with the snow telescope on Mount Wilson is built almost entirely of wood, the essential optical parts being lenses and prisms borrowed from old spectroscopes. The results secured with this instrument nevertheless surpass the best obtained by Mr. Ellerman and myself with the Rumford spectroheliograph and the 40-inch Yerkes telescope. If it were desired to do so, a complete telescope and spectroheliograph, more powerful (in combination) than any similar instruments in use, could be built for

surprisingly small sum. This possibly has been clearly demonstrated by our experiments on Mount Wilson. The same apparatus would also give results of great importance when used for high dispersion studies of the spectra of sun-spots and the chromosphere, investigations of the solar rotation, etc.

2. Professor Hale writes in answer to the questions at the end of No. 13:—

1. The Solar Observatory of the Carnegie Institution will gladly take part in co-operative work with the spectro-heliograph.

2. At least one photograph of the sun's disc will be taken on each day in each of the lines H_1, H_2, H_3, with a spectro-heliograph of 8 inches aperture and 60 inches focal length (solar image 6 inches in diameter). The chromosphere and prominences will also be photographed daily with the same instrument. In general it is expected that the dispersion of two 63° prisms of Jena glass 0·102) will be employed, but three or four prisms may be used in certain cases. A three-prism spectroheliograph of 8 inches aperture and 30 feet focal length will be used for special studies with dark lines.

3. The best definition on Mount Wilson occurs about an hour after sunrise and an hour before sunset. The principal work is therefore done at these times, but photographs will be taken at such other times as will best conform to the general plan adopted.

4. For general purposes I believe that the heliographic positions of the flocculi can be sufficiently well determined by projecting the solar image upon an adjustable globe, accurately ruled with meridians and parallels one degree apart. (A full description of this apparatus, as used to

measure photographs of the flocculi taken with the Kenwood spectroheliograph, will soon be published.) In special cases it will probably be advisable to measure and reduce the plates in the ordinary manner.

5. Records of the prominences should correspond with those published by the Society of Italian Spectroscopists.

6. We are prepared to undertake the measurement and reduction of our photographs.

7. Duplicates of our photographs can be made, if needed, for discussion with other plates at the central bureau.

8. Our daily series of photographs will include the plates named in photograph 2.

3. Professor Ricco writes:—

1. Notre spectroheliographe est en construction chez M. Toepfer de Potsdam. J'espère que nous pouvons commencer les travaux systematiques quotidiens avec l'an 1906.

2. Notre spectroheliographe est du type spectroheliographe Rumford de l'Observatoire Yerkes, c'est à dire avec deux prismes et réseau. Il s'attachera à l'extremité occulaire du refracteur de l'Observatoire de Catane qui a 0m. ·33 d'ouverture et qui donne une image focale du Soleil avec le diametre de 52 mm. Le Spectroheliographe se mouvra tout ensemble: la 1re fente devant l'image solaire, le 2de devant la plaque sensible. Le mouvement sera obtenu par un poids et sera réglé par une clepshydre. La longueur des fentes étant seulement ·32 mm., on obtiendra les photographies du disque et les photographies des protubérances en deux ou plusieures fois, ou zones.

3. Pour les conditions météorologiques de notre pays, il sera préférable de faire les photographies spectroheliographes au matin; ainsi elles seront comparables à nos observations visuelles, qui se font aussi au matin.

4. Il serait impossible pour nous de faires toutes les mesures de ces photographies, pour manque de personal. Nous pouvons envoyer des diapositifs au Bureau Central.

5. Pour à present nous engagerons à faire tous les jours clairs au moins une photographie spectroheliographique complète du disque solaire et une des protubérances, avec la fente sur la raie K; ensuite, losque nous aurons essayé notre spectroheliographe, nous saurons s'il sera le cas de nous engager à faire aussi des photographies avec une raie de l'hydrogène, etc.

4. The following Statement by Mr. C. Michie Smith has been forwarded by Dr. Gilbert T. Walker (Director General of Indian Observatories):—

1. I am ready to take part in the work and to carry out as fully as possible any programme which the Committee may approve of.

2. The spectroheliograph is fed by an 18-inch mirror and siderostat of the Foucault pattern. The lens is a 12-inch Cooke photo-visual lens of about 20 feet focus. In front of the first slit there is a collective lens of $3\frac{1}{2}$ inches aperture. The collimating and camera lenses are of 5 inches aperture and 6 feet focal length, and are not achromatic. Two 60° prisms are always used. At an early date I expect to receive a grating which can be substituted, when required, for the present plane mirror. A negative enlarging lens has just been received with which parts of the disc can be photographed on a scale of 7 inches to the sun's diameter. On each clear day I am prepared to take photographs in H_1 and H_2 for the disc, H_2 for the limb, and one or two other lines as may be thought best. Of course there will be a good many days on which bright sunshine will not last long enough to permit of the whole programme being carried out, and I

think the photographs should as a rule be taken in the order given above. When the ordinary photographs show regions of special interest, enlarged photographs of these parts should be taken.

3. As a general rule the most suitable time of day for solar photographs here is from about 8h. to 10h. M.M.T., or, say, from 2h. 30m. to 4h. 30m. G.M.T. On favourable occasions work may continue for one or even two hours more, and during part of the year fairly steady images can be got in the afternoon. For regular work one can only count on the early forenoon hours.

4. The method of measurement adopted by Professor Hale at Yerkes, in which the photograph is projected on to a globe seems to me far the best for work of this kind, so long as the photographs give a truly circular image of the sun.

5. In spectroheliographs which are fed by a Foucault siderostat the rotation of the image introduces complexities in the reduction of the prominence photographs. Personally, I have found it best to determine visually the positions of one or more prominences and to use these for orienting the negative. Where long exposures are used due allowance must be made for rotation during the exposure, but this is easily done if the position of a prominence is determined near the point on the limb where the exposure begins and another near the point where it ends. The best method of tabulating the results is a subject deserving some attention, but I am not at present prepared to make any definite suggestions. It is probable that it would be well to draw up a series of types of prominences for purposes of classification, and though there is an endless variety in individual prominences a very moderate number of types would probably be sufficient. The mere tabulation by latitude, height and

extent of base seems quite insufficient to give a true view of the relative importance of the prominences. It might be well to measure the total area of the photograph of the prominences, but to do this satisfactorily would involve a very considerable amount of labour, while it seems doubtful if anything is gained by making more than the roughest estimates of areas which change so rapidly.

6. For the detailed measurement of the flocculi there would be many advantages in having the work done at a central bureau. In most cases it would probably be possible to provide a duplicate set of negatives for this purpose, and I would be willing to do this so far as possible. There are, however, many other investigations to be made requiring as complete sets of negatives as possible, and I would be unwilling to part permanently with negatives of which I had no duplicates. This difficulty could be got over in several ways. For such investigations as I am contemplating positives would probably be found nearly as good as the negatives, and it could be arranged that the original negatives should be returned after they had been measured. I could not begin to supply negatives without some notice, since all the photgraphic plates have to be received from England through the Indian Store Department, and the supplies at present ordered would not be sufficient if a regular series of duplicates is to be taken.

7. Pending the adoption of a definite programme I am prepared to take daily photographs in the *H* line of both disc and prominences, and, as soon as I receive the grating, to take in at least one other line.

5. Mr. Valle writes:—

I have not been able to answer the circular on the International Union for Co-operation in Solar Research

before this day. But now I have the pleasure to state that this Observatory will gladly co-operate in the Solar Researches, and I beg you to have the kindness to inform the Committee of this resolution.

We have not yet the instruments that we want for that work, but I have asked for a Solar spectrograph, and I am already in the preliminary talkings to order a spectroheliograph to be mounted in our 15in. equatorial which gives an image of the sun about 55 mm. diameter.

I hope to receive the spectroheliograph by the end of this year or a little later, but we will begin work immediately. I hope also, in the near future, to have mounted in our coelostate a large mirror, and in connection with a suitable lens to obtain images at larger diameter than 55 mm.

I cannot fix for the moment a definite programme of work, but you may be sure that our observations shall be made subject to the programme that the Conference or meeting at Oxford in September next will fix. Unluckily I shall not be able to be present there in September, but I agree beforehand in anything that the Conference might approve.

I will do my best to send in my name the assistant-in-chief or sub-director of this Observatory. He is at this moment in Spain with the party of this Observatory sent to observe the next eclipse of the sun, and I hope will not find any difficulty in leaving for England in time to be present, on my behalf, at the meeting to be held at Oxford.

The best time for solar work here is between 8 and 10 in the morning. Generally in the evening hours the radiation currents are very strong and definition very poor, notwithstanding the great altitude of the country. Besides that in the summer (and rainy) season the fall hours are very

cloudy after 3 p.m., but when the weather is fine the definition is magnificent.

I will try to buy all instruments to measure here at least some plates, but I judge best to make measures and reductions in a central bureau, whenever possible, and consequently I will send negatives or at least copies to the central bureau for reduction and further examination. As we have no experience at all in that kind of work I cannot make any suggestions useful to the Committee. I will state, furthermore, that I hope to have taken daily at least two photographs at different hours, and also as often as necessary whenever a great perturbation be present in the sun's disc. We will try also the radiations K, H, and K_2 and H_2 and any others found suitable for the purpose.

6. Professor Abbot writes:—

Spectroheliographic observations seem conspicuously suitable to reveal minutes changes in the appearance of the sun such as might readily be connected with changes in solar radiation to be mentioned under (3), and which may in the end serve indirectly to indicate the time, direction, and amount of such changes in solar radiation independently of direct observations of the solar radiation and absorption. I would therefore urge the importance of the continuous spectroheliographic survey of the sun so well outlined by Professor Hale, not only from the standpoint of interest in the knowledge of solar phenomena in themselves, but also for their probable future meteorological interest; and I would urge the careful comparison of the spectroheliographic and radiometric results in order to determine without undue delay whether such a connection between the appearance and the radiation of the sun, as above suggested, may be discovered. Spectro-

heliographic observations of the past, however fragmentary, may then become of new interest and value to inform us of the amount of solar radiation in former years.

7. Professor Frost writes:—

As to work with the spectroheliograph, I will follow the numerals used in the circular drawn up by Professor Hale:

(1) The Yerkes Observatory is prepared to take part in the work.

(2) The apparatus has been sufficiently described in the *Astrophysical Journal* and in the "Publications of the Yerkes Observatory." It is our plan to take on each clear day photographs of H_1, H_2, and of the prominences.

(3) 9 to 12 a.m. and 1 to 3 p.m. Observations could be made at other hours in summer, but not in winter.

(4) The method of measurement and reduction introduced here by Professor Hale with the use of a globe performs satisfactorily.

(5) For measurement, I should suggest a protractor for position angle, and a small transparent scale for measuring height.

(6) We shall undertake the work of measurement and reduction at the Observatory.

(7) It would not be possible to make duplicate negatives to send to a central bureau; but prints could be made and sent, or possibly contact copies on glass.

(8) Has been answered under (2).

(9) As circumstances permit, observations will be made upon interesting lines other than those of calcium and hydrogen.

IV. Spectra of Sun-spots.

1. Professor Hale writes:—

I have expressed my views elsewhere regarding the importance of co-operation in observations of the spectra of Sun-spots.[1] It is obvious that a simple understanding among the various observers regarding the general scheme of observations and the regions of the spectrum to be covered by each would result in decided advantage. While a small solar image, observed with a spectroscope of moderate resolving power, will give results of great value, attention should also be directed to the importance of observing visually the details of spot spectra with more powerful spectroscopes in conjunction with large equatorials. Many large telescopes, which are not now in use during the day, would give valuable results if provided with inexpensive grating spectroscopes of simple design. For the photography of the widened lines I believe that solar images at least 6 inches in diameter should be available. Since spectroscopes of great focal length are also needed in this work, some form of coelostat telescope is likely to prove most suitable.

In the discussion of observations of spot spectra, special attention may well be directed to their correlation with stellar spectra and with the results of laboratory experiments. There is an excellent opportunity for co-operation among investigators in these three fields of research.

The Solar Observatory will co-operate with other institutions in observations of the spectra of Sun-spots. Pending the completion of certain investigations now in progress, however, our definite programme cannot be de-

[1] Address before the St. Louis Conference on Solar Research, *Astrophysical Journal*, vol. xx., p. 310, 1904.

termined. It is probable that the observations will be made entirely by photographic means, with a solar image 16 inches in diameter, and a long focus grating spectrograph. A full statement will be published later.

2. Professor Fowler (Royal College of Science, London) writes:—

1. So far as other duties permit I shall be glad to co-operate with other observers in recording the spectra of sun-spots. I could promise continuous observations from October to February inclusive, and a fair number of observations from March to July.

2. The intruments at present at my disposal are a 6-inch Troughton equatorial (76 inches focus, giving an image about 0·7 inches diameter) and an Evershed solar spectroscope. The spectroscope gives very fine definition, and I clearly divide lines of moderate intensity differing in wave-length by 0·28 tenth-metre in the region between *b* and *E*. The practical purity of spectrum in this region may accordingly be put at about 20,000, and other data give the corresponding resolving power as 60 to 70 thousand. In connection with widened lines, however, it is sometimes possible to attribute the widening to a particular component of a double which cannot be clearly resolved, by noting on which side the widening chiefly lies.

3. My equipment seems to me to be best adapted for work in the green, and especially in the region *b* to *E*. The dispersion is too small to be perfectly satisfactory in less responsible parts of the spectrum, and beyond *b* the spectrum is so greatly complicated as to need the highest possible dispersion. From *E* to b_4, 40 to 50 affected lines are noted in a complete observation, but it is probable that

some of these are part of the structure of the general spot band.

4. The wave-lengths of the affected lines are first read off approximately by comparison with Rowland's photographic map, and are afterwards corrected by reference to Rowland's tables.

5. I am entirely in favour of the observations being collected by a central bureau.

6 and 7. The fulness of the records will necessarily be determined by the number of observers, but I am of opinion that a complete record of a limited part of the spectrum should be aimed at, rather than a record of only the principal lines over a more extensive region. As to the extent of spectrum to be undertaken by each observer, I think that the region b to D should be divided among at least four observers, and that the regions should overlap so as to give a test of uniformity.

The principal suggestion that I wish to make refers to the scale by which the amount of "widening" may be most usefully expressed. Seeing that a knowledge of the intensities, as well as of the wave-lengths of the lines, is essential to the proper interpretation of any spectrum under investigation, it seems to me of the greatest importance that some uniform mode of stating the relative intensities of the lines in spots should be adopted. As I have already suggested,[1] a convenient method is to compare the spot lines with the Fraunhofer lines in the immediate neighbourhood, and to take the corresponding intensities from Rowland's tables. I am glad to note that the advantages of this procedure have already been recognised by Mr. W. M. Mitchell, of Princeton.[2]

[1] Monthly Notices, R.A.S., vol. lxv., pp. 214, 515.

[2] *Astrophys. Journ.*, vol. xxii., p. 7.

Addendum.

I consider it desirable that each observer should make a practice of noting the behaviour of C, F, and D_3 in and near the spots, and that some steps should be taken to secure co-operation in recording the bright lines seen in disturbances on the sun's limb, whether accompanied by large prominences or not.

3. Mr. C. Michie Smith writes:—

1. I am ready to co-operate in a systematic study of the lines affected in sun-spot spectra.

2. The work on spot spectra is at present being carried on here with a 3-prism Evershed spectroscope attached to the Cooke Equatorial of 6 inches aperture and 7 feet focus. Under favourable conditions this will separate the lines 5066·078 and 5066·174.

3. The main work is done on the region D to F, but when time is available additional observations are made in the region C to D. I can not at this moment state how many widened lines are included, but a catalogue of them will soon be prepared.

4. Widened lines are identified by reference, at the telescope, either to Rowland's or to Higg's maps of the region.

5. At present these observations are published as early as possible in Bulletins of the Observatory. It seems to me the best way would be to continue this plan and to forward early copies or even proofs to the central bureau for such use as they may see fit to make of them. I would, if thought best, publish a list every six months, and I hope that before long it will be possible to issue these bulletins very soon after the close of the period to which they refer.

6. As soon as I get additional help in the shape of an (European) assistant I hope to take up the systematic study

of sun-spots photographically. For this purpose I have obtained a very high-class grating ruled on a parabolic surface of 10 feet stellar focus. This I propose to use on an image formed by a 6-inch lens of 40 feet focus. A collimating lens of 8 or 9 feet focal length will be used. If this arrangement does not prove satisfactory, I will use a plane grating with collimating and camera lenses of 8 feet focus. There does not seem to be any real difficulty in obtaining satisfactory photographs of sun-spot spectra when the image of the sun formed on the slit of the spectroscope is sufficiently large, but as my work so far has been only experimental I would prefer not to go into details in case more detailed work should show that I have been too sanguine. I trust, however, that next year I will be in a position to give the Committee more information on this point.

In conclusion I would say that I am ready to do everything in my power to forward the objects of the International Union.

4. Mr. Julius C. Fényi, S.J., writes:—

In Beantwortung der an mich gerichteten Zuschriften betreff der Mitwirkung zur Erforschung der Sonne—"Co-operation in Solar Research"—, teile ich mit, dass ich meinen beschränkten Mitteln gemäss bereitwilligst teilneme. An der Spectroheliographie kann ich mich nicht beteiligen, weil mir kein Spectroheliograph zur Verfügung steht. Programmässig wird am Haynald Observatorium schon seit 1885 der Sonnenrand mit einem automatischen Spectroscop, bestehend aus 6 Flintprismen, welche zweimal vom Strahle durchlaufen werden, beobachtet. Die Protuberanzen werden gezeichnet und deren Höhe und Position gemessen; dabei werden auch eruptive Erscheinungen, sowohl am Sonnenrande, als auch auf Sonnenscheibe, wenn

solche über den Fleckenherd vorkommen, sorgfältig beobachtet; die Spectrallinien wurden bisher nach dem Atlas von Angström, nur behuss der Identificirung bestimmt.

Auf die im besonderen über die Beobachtung der Spectra der Sonnenflecke vorgelegten Fragen habe ich mitzuteilen, dass der Beobachtung der Umkehrung mancher Linien schon bisher Sorgfalt zu gewendet wurde; auch über die Verbreiterung macher Linien über den Kernflecken wurde zu untersuchen versucht. Auf die einzelnen Fragen habe ich folgendes zu antworten:

(1) Ich bin bereit an der Beobachtung der Linien über den Sonnenflecken teil zunemen.

(2) Mein Instrument ist ein 7″ Refractor von Merz mit automatischem Spectroscop von Hilger. Collimator und analysator—linse haben 25 mm. Durschmesser und 19 cm. Fokusweite. Das Sonnenbild im Fokus des Refractors hat 20 mm. im Durchmesser; die Dispersion von *A*—*H* beträgt 60°; gewöhnlich wird mit der Vergrösserung 85 beobachtet.

(3) Ich hätte die Absicht die Linien zwischen *C*—*D* besonderer Aufmergsamkeit zu unterwerfen; ich könnte allenfallse auch ein Strecke bis zu *b* wählen.

(4) Ich pflege das Spectrum mit dem Atlas von Ångström zu vergleichen.

(5) Ich bin bereit die Resultate dem Centralbureau mitzuteilen.

(6) Ich habe nur die Absicht grössere Flecke zu untersuchen; dies kann auch nur nach Vollendung der programmässigen Beobachtung der Protuberanzen geschehen, wenn noch Zeit ist.

Eine Mitarbeit kann erst mit nächstem Jahre beginnen weil die Sonnenfinsterniss für dieses Jahr mich bis zum Herbst in Anspruch nimmt.

5. Professor Ricco writes:—

1. Nous n'avons pas assez de personel pour nous engager dans un travail systematique de ce genre, nous pourrons faire des études occasionelles, sans engagement formel.

2. Nous avons un spectrographe Vogel, qui appliqué au refracteur Merz on a l'equatorial Cooke (ouverture $0^{m}.15$, longueur focale $2^{m}.23$) nous donne les bonnes photographies due spectre de taches solaires: la dispersion est mediocre (de F. à K. 28 mm.).

V. Solar Rotation.

1. The following communication has been received from Mr. Walter S. Adams:—

In connexion with the September meeting of the International Committee, at which the general programme of work will no doubt come up for discussion, I wish to call attention to certain problems of a spectroscopic nature which would seem worthy of consideration at that time.

The first of these is that of the determination of the period of the solar rotation by means of spectroscopic observations. Photographic methods, which have totally changed the character of solar and stellar spectroscopy within recent years, have not as yet been applied to this investigation, and in spite of the high degree of accuracy attained in the results of Dunér and Halm, there is every reason to believe that a considerable gain can be made by the substitution of photographic for visual measures. It is of the utmost importance, moreover, that the question of the character of the results given by lines due to different elements be examined, as well as those given by different lines of the same element. Up to the present time but two lines have been employed, both of them due to iron, and lying within a tenth-meter of one another in the red region

of the spectrum. By the aid of photography, chromospheric lines can also be investigated, and the results would go far towards settling many of the perplexing questions connected with this region of the solar atmosphere.

The recent work of Halm has given some evidence of a change in the period of rotation of the reversing layer with the state of the sun's activity, and the importance of a confirmation of this result can hardly be over-estimated, being absolutely vital in its bearing upon solar theories. It is clear that the amount of labour involved in a thorough discussion of this problem is very great, and will require the co-operation of all observatories possessing apparatus of the requisite power. At the same time it is difficult to find one which can have wider application to all sides of solar research. The spectroscopic method would render possible the detection of slight changes in the period of the solar rotation which would certainly escape notice in observations of spot or faculæ positions.

A second investigation, bearing both upon questions of wave-length and the problems of the solar constitution, is that of the relative displacements between arc and solar lines. The only work of importance in this direction is that of Jewell, and it is most desirable that this should be confirmed and extended. In many cases his results are based upon but one or two measures, and the accidental error bears a large ratio to the size of the quantity measured. The necessity of extensive measurements of these displacements has been remarked upon by Hartmann in his discussion of standards of wave-length. Their importance in showing the pressure of the various vapours in the sun's atmosphere, and consequently the altitude at which the lines are formed, is self-evident. An additional subject in which a knowledge of the magnitude of these displacements is most desirable is stellar spectroscopy. In

the absence of sufficiently extensive tables of the wave-lengths of arc and spark lines, it has commonly been the practice to use the solar spectrum wave-lengths for both the comparison spectrum and the stellar lines. The full value of the displacements accordingly enter into these results as an error, except in so far as they are counteracted by the arbitrary corrections which were applied in the construction of Rowland's tables.

A problem intimately connected with the preceding is that of the application of Doppler's principle, with a view to determining the motion in the line of sight of the gases composing the sun's atmosphere. An illustration of the value of such determinations is given in the case of the *H* and *K* lines of calcium. When these lines are used in connection with the spectroheliograph the distribution of calcium vapour over the sun's surface is determined, and, with the more powerful instruments, the distribution of that part which lies along the line of equal pressure, presumably at the same level in the sun's atmosphere. The knowledge of the direction of motion of this vapour would be invaluable to a satisfactory discussion of spectroheliographic results.

Without entering further upon the desirability of spectroscopic measures of this general character, I would, in conclusion, merely lay emphasis upon their essentially quantitative nature. In all branches of science, when a considerable advance is made in the accuracy with which we know the numerical data of phenomena, a great number of important facts come to light which necessarily remained unthought of previously. In the case of solar phenomena, such an advance can probably be made only through extensive spectroscopic measures, and the results of these determinations will be the material upon which adequate solar theories of the future must be based.

2. Dr. Halm has also written to urge the determination of the period of solar rotation by means of spectroscopic observations.

VI. Co-operation and General Matters.

1. Dr. Gilbert T. Walker (Meteorological Reporter to the Government of India and Director General of Indian Observatories) writes:—

1. On behalf of Mr. Michie Smith and myself, I beg to thank the Executive Committee of the International Union for Co-operation in Solar Research and the authorities of New College for their kind invitation to the Conference at Oxford, on September 27th, and to express our great regret that we shall not be able to attend.

2. In relation to the intensity of solar radiation I enclose some suggestions for work to be done in this place and in one or more of our largest meteorological observatories in northern India.

3. As far as astronomical research is concerned the equipment of the department is concentrated at Kodaikanal, and I forward a copy of the suggestions of the Director of that Observatory; these I fully endorse, and shall be glad to support as far as circumstances will allow.

4. The International Union will be of the greatest advantage to us in India, and our co-operation to the best of our ability may be relied upon.

2. Professor Wolfer (Zürich) writes:—

Obschon die Züricher Sternwarte nicht die Hülfsmittel besitzt sich direct an den neuen spectroheliographisten

Arbeiten zu beteiligen, nehme ich doch das lebhafteste Interesse an diesen, weil die Beobachtungen die ich seit 1887 hier fortgeführt habe, in enger Beziehung zu jenen stehen, insofern sie wenigstens für die Sonnenfackeln einen Teil dessen bieten, das künftig durch spectro-heliographische Methoden in weit vollkommener und vollständigerer Weise geliefert werden wird.

Dieses Interesse wird auch dann nicht geringer werden, wenn durch die neuen Methoden meine bisherigen Beobachtungen unnötig gemacht werden sollten, was ich trotz der gegenteiligen Meinung die mir Professor Hale hierüber aüsserte, für wahrscheinlich halte. Um so mehr Befriedigung würde es mir gewähren, mich in diesem Falle in anderer, vielleicht indirekter Art an den zu vereinbarenden gemeinsamer Arbeiten zu beteiligen.

3. Mr. David Hadden writes:—

Replying to your communication of May 5th ult., relative to an International Union for Co-operation in Solar Research, I will say that I shall be glad to co-operate as far as I am able.

My solar work during the past thirteen or fourteen years has been mainly visual, consisting of a daily record of the groups, spots and faculæ, also visual observations of sun-spot spectra, and occasional spectroscopic work on prominences. My results have been contributed regularly to the Director of the Solar Section of the British Astronomical Association.

I will be pleased to co-operate and undertake any special line of observations connected with sun-spot spectra which I can do with my present instrumental equipment.

No. 15.

Two Letters addressed by Prof. Callendar to the Editor of the "Astrophysical Journal," and printed in that journal, vol. xxiii., pp. 167 and 408.

Sir,—My attention has been called to a statement by Mr. G. T. Walker, contributed to the Oxford meeting of the International Union for Co-operation in Solar Research, with regard to one of my Sunshine Recorders, which does not appear to represent the facts. I had no opportunity of correcting this statement at the time, and it has received very wide circulation among those interested in the subject without contradiction. In view of the importance of the full discussion of all promising methods of recording solar radiation, I trust you will be able to allow me space for a reply.

Mr. Walker complains that a particular instrument of the old type with removable cover, made in 1898, has been damaged by condensation of water vapour, that the black has peeled off the wires, and that the constants of the instrument have altered. I have looked up the case of this particular instrument, and can only conclude that Mr. Walker has been misinformed. I find a letter from the observer in charge, dated February, 1902, stating that the apparatus was working extremely well. Nearly two years later it was returned to England for the re-determination of its reduction factor, which the makers had omitted to supply in the first instance, owing to some misunderstanding. The instrument was found to be in perfect condition, the coating of black on the wires was quite uninjured, and there was no evidence whatever of any alteration in its reduction factor. We do not, as a matter of fact, recommend this type of receiver for use

in damp climates, because, not being hermetically sealed, there is risk of damage from condensation, in case the observer in charge omits to renew the drying material periodically according to instructions. The hermetically sealed type is certainly preferable in this respect, but is open to the objection that it cannot so easily be repaired in case of damage in transit. But if, as Mr. Walker states, water was allowed to collect and drop on the wires, it is certainly surprising that the instrument should have endured such treatment for so long a time without any evidence of alteration. It seems unfair, however, to blame the instrument or condemn the method.

Yours truly,

(Signed) H. L. Callendar.

Royal College of Science, London, S.W.,
October 26th, 1905.

Sir,—With reference to my letter of October 26, 1905, which appeared in the March issue of your Journal, I have since had the pleasure of a personal interview with Mr. G. T. Walker, which has resulted in a simple explanation of the difficulty experienced with the sunshine receiver in question. Mr. Walker informs me that there was a good deal of condensation inside the cover of the receiver at the time when he saw it, which would naturally render the results unreliable, but that there was no drying material in use at the time, and that neither he himself nor the observer in charge was aware of the necessity of employing drying material to prevent condensation. It appears on investigation that the Instrument Company which supplied the receiver, by some accidental oversight, owing to the different parts of the apparatus having been ordered and supplied at different times, omitted to send

specific instructions with regard to the necessity of employing drying material in the receiver. I greatly regret that this omission did not come to my knowledge sooner, but I had no personal knowledge of the transaction, and the Instrument Company had some difficulty in finding the records of the correspondence, etc., relating to the subject, as the instrument was supplied so long ago. Mr. Walker assures me that he has no reason to believe that the method would not give good results, if the receiver were kept dry, but he agrees with me that the hermetically sealed type would be more suitable for use in damp climates, as requiring less attention; though even in this case it is necessary to clean the exterior of the receiver at frequent intervals, if exposed to dust or damp. As several of these instruments are now in use in various parts of the world for investigating possible variations of the solar radiation, it is important to draw attention to the necessity of these precautions, as other observers may have experienced similar difficulties.

H. L. Callendar.

Royal College of Science, London S.W.
April 3, 1906.

No. 16.

Communication from Mr. W. A. Michelson.

Mr. A. Bélopolsky sent me your circular on the co-operation in researches on "Intensity of Solar Radiation." As I have been interested in actinometry for more than twelve years, I venture to submit to the International Committee on Solar Research the following considerations:—

In January, 1893, I constructed my first "ice-pyrhelio-

meter," which is founded on the principle of the "perfectly black body" as absorber, and consists of an adaptation for actinometric purposes of the Bunsen's ice-calorimeter, with a blackened inside tube and a narrow, round diaphragm, the whole provided with an altazimuthal mounting.

This instrument is described only in the *Journal of the Russian Physico-Chemical Society*, January, 1894, and this may be one of the causes why nobody else tried the same method afterwards. The results which I obtained in March, 1893, in Davos (Switzerland) and later in Russia, seemed very promising.

At present the principal features of my ice-pyrheliometer remain unchanged, but I have given it some technical improvements, like platina absorption tubes, very fine divided tube, etc.

Last year I received from my friend, Professor K. Ångström, a very fine specimen of his electrical compensation pyrheliometer, and now I am working to the purpose of comparing these two instruments. Unfortunately the weather at Moscow was very unfavourable last summer and this spring. Moreover I had some difficulties in overcoming the effect of capillarity on mercury in the narrow calorimetrical tube. So I have not yet got sufficient results to give a full account of these comparative measurements. But I hope I shall carry them out in the coming spring of 1906.

Other so-called "actinometers," besides the two mentioned are perhaps of some utility in meteorology, but certainly cannot be taken in account for astrophysical purposes.

As regards your point 4, I agree that the methods of reduction, taking into account the influence of the Earth's changing atmosphere, offer the most difficult portion of

the problem. But the two methods mentioned by you, that of two levels and that of one level and different Sun-heights, are, it seems to me, not the only possible ones. I hope soon to propose a quite different and more empirical method of reduction, which has the great advantage that measurements made at many different stations, and perhaps at quite different times, can be used together. But I dare not invite the attention of the International Committee to this method before having tried it on a sufficient number of my own measurements.

Moscow Agricultural Institute,
16th September, 1905.

No. 17.

Communication from Mr. Evershed.

I much regret that I shall not be able to attend the Conference of the International Union for Co-operation in Solar Research, to be held on the 27th inst.

In reading through the published suggestions for consideration at the meeting which you kindly sent me, I am struck by one seeming omission from the programme of observations relating to sunspots to which I should like to call attention. It has reference to the fundamental question, of the origin of the darkness of sunspots. Spots have been observed telescopically for 300 years, yet do we know any more than Galileo why they are dark? It seems to me that the observation of details, such as widened lines in their spectra, is of secondary importance compared with the determination of the cause of the darkness. A searching enquiry as to the real nature of the apparently continuous dark band of the spot spectrum, I

suggest, ought to come first on the programme of a well-equipped solar observatory.

The striking discrepancy between thermal and visual estimates of spot darkness requires explanation, and I would suggest that estimates of the darkness compared with the adjacent photosphere should be made in all accessible regions of the spectrum, by spectro-bolometric, visual, or photographic methods.

It may be recalled that in general the relative darkness of spots compared with the photosphere is much greater when estimated visually, or photographically, than when measured thermally by the bolometer or the radio-micrometer. That is, spots appear to be less black than when the entire visible and invisible spectrum is integrated than they do when the visible rays alone are used; and this is most marked with spots situated near the sun's limb, in which position both Frost and Langley have measured umbræ, in which the thermal intensity actually exceeded that of the neighbouring photosphere.

It becomes then of paramount importance to find out where in the spectrum is this excess of radiation? It is certainly not within the limits of the visible spectrum. Is it possible that the thermal measures hitherto made have been seriously affected by diffuse reflexion of photospheric radiation by the atmosphere, and from the surfaces of heliostat mirrors, etc.? New determinations of thermal radiation ought, I think, to be undertaken, and at high altitudes, such as Mount Wilson and at Kodai-Kanal, and, if possible, without the use of mirrors in forming the primary image.

It appears to be generally admitted now that a spot is a region where the photospheric clouds have been evaporated by a local access of heat; yet the spot band is spoken of as an *absorption* band. Is it absorption or diminished

radiation? Surely this primary question ought to be settled once for all. The resolution of a limited portion of the spot band into alternate dark and bright rulings recently re-observed by Fowler, appears to bear directly on this question, and gives a lead for further investigation with the very powerful instruments now available.

KENLEY, SURREY,
September 22nd, 1905.

No. 18.

VORLÄUFIGES PROGRAMM DER RUSSISCHEN ABTEILUNG DER INTERNATIONALEN SONNENCOMMISSION.

(Aus dem Russischen übersetzt.)

I. Untersuchungen der Sonne, zu deren Ausführung es der Cooperation vieler Staaten bedarf und an denen auch Russland teilzunehmen bereit ist.

II. Untersuchungen, die der privaten Initiative überlassen werden müssen, bei denen jedoch das Internationale Comité durch Ratschläge und Zuwendung pecuniärer Mittel, sowie durch Bearbeitung und Druck der Resultate mitwirken kann.

Zu Punkt I gehören die sich mit Untersuchung der Sonne befassenden Arbeiten, welche bereits in Russland ausgeführt sind oder aber von russischen Sternwarten übernommen werden können, nachdem Russland seine Bereitwilligkeit erklärt hat, am internationalen Unternehmen, das sich die Erforschung der Sonne zur Aufgabe gemacht hat, teilzunehmen. Derartige Arbeiten wären:

(1) Photographische Untersuchungen, d. h. die Herstellung täglicher Aufnahmen der Sonnenscheibe in kleinem (bestimmten) Massstabe für die Statistik der Sonnen-

flecken und zur Ermittelung ihrer heliographischen Lage zum Zweck der Ableitung der Elemente der Sonnenrotation.

Anmerkung: Vorgeschlagen wird:

(*a*) Aufnahmen, die sich über einen bestimmten Zeitraum (z. B. über eine ganze 11 jährige Periode der Sonnentätigkeit) erstrecken, an einen vom Internationalen Comité bezeichneten Ort zur definitiven Bearbeitung zu senden; oder

(*b*) an einem Ort alle Platten anzusammeln, die sich auf ein bestimmtes Gebiet der Sonnenuntersuchung beziehen (z. B. alle photoheliographischen Aufnahmen),

(*c*) die besondere Aufmerksamkeit des Internationalen Comités auf die Ausarbeitung eines gemeinsamen Schemas zu lenken, nachdem die Messungen ausgeführt und die Resultate berechnet werden sollen.

(2) Die detaillierte Erforschung einzelner Teile der Sonnenoberfläche mit Hilfe grosser Fernrohre (in grossem Massstabe).

Es müssten bestimmte Zeitmomente vorher vereinbart werden, zu denen derartige Beobachtungen erfolgen sollen, und, wenn besonders interessante Erscheinungen vorliegen, alle Beobachtungsorte telegraphisch benachrichtigt werden.

(3) Spectraluntersuchungen, Spectralanalyse der Flecken, Studium der Verschiebung und Erweiterung der Linien.

Dazu müsste eine Vereinbarung inbetreff der Ausführung der mit einander zu vergleichenden Untersuchungen getroffen werden. Am zweckmässigsten wäre es, jeder Sternwarte ein Stück des Spectrums zur Untersuchung

zuzuweisen, dabei aber einen bestimmten Teil, um eine Vergleichung zu ermöglichen, allen gemeinsam sein zu lassen.

(4) Spectroheliograpische Untersuchungen der Sonne.

Die russische Commission hält diese Untersuchung für besonders wichtig und wäre gern bereit, an ihr teilzunehmen, wenn die Möglichkeit vorläge, sie zu verwirklichen, kann aber zur Zeit keine bestimmten Zusagen machen, solange die dazu erforderlichen Mittel nicht vorhanden sind.

(5) Der russischen Abteilung der Internationalen Commission scheint eine Cooperation bei der Ausführung möglichst häufiger absoluter aktinometrischer Beobachtungen zum Zweck der Bestimmung der "Sonnenconstante" und der wahrscheinlichen Schwankungen ihres Betrages für besonders wünschenswert, sie spricht aber in Ermangelung eines Instrumentes, das allen Anforderungen entspricht, die an eine vom astrophysikalischen Standpunkt genaue Ausführung aktinometrischer Bestimmungen gestellt werden können, und geeigneter Methoden, um die Wirkung der Erdatmosphäre auszuschliessen, den Wunsch aus, das Internationale Comité möge Hinweise geben, welche Massnahmen zur Erreichung des obenangeführten Zweckes zu ergreifen wären.

(6) Die russische Commission hält es für unbedingt notwendig, ins Programm der allgemeinen internationalen Untersuchungen der Sonne auch die Beobachtung totaler und ringförmiger Sonnenfinsternisse zu astrophysikalischen, physikalischen und meteorologischen Zwecken aufzunehmen und äussert den Wunsch, dass das Internationale Comité dazu ein Programm ausarbeite und bei der Organisation von Expeditionen mitwirke.

II. Ausser diesen Aufgaben, bei denen ein internationales Zusammenwirken unbedingt erforderlich ist,

giebt es noch viele andere, die von einzelnen Personen übernommen werden können. Derartige, zum Teil sehr wichtige und ihrer Lösung harrende Aufgaben sind aus verschiedenen, zum Teil zufälligen Ursachen bisher unbeachtet geblieben. Daher wäre es sehr zu wünschen, dass das Internationale Comité auf dieselben aufmerksam machte, auf ihre Bedeutung und Ausführbarkeit hinwiese und ihre Mitwirkung bei ihrer Ausführung zusagte und zwar:

(1) durch Angabe der Untersuchungsmethoden und der nötigen Instrumente;

(2) durch Beschaffung der Mittel zu ihrer Ausführung;

(3) indem sie Mitarbeiter zu werben sucht und die geeignetsten Beobachtungspunkte ausfindig macht;

(4) durch die Bearbeitung und den Druck der Resultate.

Um die Auswahl solcher Aufgaben zu erleichtern, wäre es wünschenswert, dass jeder Astronom oder Physiker dem Internationalen Comité diejenigen sich auf die Sonnenphysik beziehenden Themata zur Veröffentlichung mitteilte, welche er bei dem gegenwärtigen Stande der Wissenschaft für wichtig und ausführbar hält, zu deren Bearbeitung es ihm selbst aber an Instrumenten, Mitteln oder Zeit gebricht, und dass derartige Themata in irgend einem periodischen Journal (z. B. d. Astrophysical Journal) gedruckt würden.

Solche Aufgaben wären zum Beispiel:

(1) Die Bestimmung der Temperatur des Kerns der Flecken nach der von Prof. Lebedeff angegebenen Methode.

(2) Die Bestimmung der Grösse des Sonnendurchmessers in verschiedenen Richtungen nach Prof. Ceraski's Methode.

(3) Die Bestimmung der Sonnenrotation in verschiedenen Breiten aus der Verschiebung der Spectrallinien.

(4) Die Bestimmung der untersten Grenze der Sonnentemperatur mit Hilfe concaver Spiegel nach Prof. Ceraski's Methode.

(5) Untersuchungen im Laboratorium von Metalldämpfen bei Temperaturen, die höher liegen als die kritischen.

(6) Die Untersuchung der Gesetze der Sublimation, d. h. des unmittelbaren Ubergangs der Körper aus dem festen Zustande in den gasförmigen.

(7) Die Construction und Handhabung eines absoluten Aktinographen, der auf dem Princip des absolut schwarzen Körpers basiert ist nach Prof. Wlad. Michelsons Vorschlag.

(8) Die Untersuchung der unteren Schichten der Chromosphäre mit Hilfe eines runden Spalts.

No. 19.

Communication from the Imperial Academy of Science in Vienna as Leading Academy of the International Association of Academies.

Am 3. Juni 1904 schrieb uns Herr George Hale in Pasadena in Californien, es habe sich eine Anzahl von Observatorien und Gelehrten vereinigt, gemeinsame Untersuchungen über die Sonne zu veranstalten. Zugleich wurde die Kaiserliche Akademie eingeladen, zu dem gleichen Zwecke ein Komitee zu bilden und ein oder mehrere Repräsentanten dieses Komitees zu einer im September desselben Jahres zu St. Louis abzuhaltenden Beratung zu entsenden. Zu jener Zeit lag die Geschäftsführung der Internationalen Assoziation nicht in der Hand

der Wiener Akademie, sondern in jener der Royal Society. Da dieses Unternehmen ausserhalb der Liste der Unternehmungen der Assoziation lag, wendeten wir uns um gefällige Auskunft an massgebende Persönlichkeiten in London, und wir einigten uns dahin, einen Vertreter nach St. Louis zu schicken, zugleich aber, dieses Comittee on Solar Research zu ersuchen, es wolle von dem Fortgange seiner Arbeiten der Assoziation von Zeit einen Bericht zukommen lassen.

Als unser Vertreter in St. Louis ist Hofrat Boltzmann erschienen. Der dort gefasste Beschluss ist über unsere Anregung hinausgegangen; er wurde vom Professor Poincaré gestellt und lautet:

> "que le comité nommé entre en negotiation avec l'Académie des sciences de Vienne pour demander l'approbation et le patronage de l'association internationale des Académies."

Es ist unsere Pflicht zu erinnern, dass am 25. Mai 1904 zu London (siehe Report of Proceedings page 7) der Beschluss gefasst worden ist:

> "Dass die Begründung irgend einer neuen internationalen Organisation, die von verschiedenen Staaten unterstützt werden soll, zuerst sorgfältig hinsichtlich ihres Wertes und ihrer Objekte geprüft werden muss und dass es wünschenswert ist, dass Anträge, solche Organisationen einzurichten, vor der definitiven Ausführung von der Internationalen Assoziation der Akademien erwogen werden."

Diesem Beschlusse zufolge sind wir auch heute nicht in der Lage, dieses Unternehmen in die Liste der Unternehmungen der Assoziation aufzunehmen, aber wir sind der Meinung, dass dieser Umstand dem Unternehmen selbst nicht abträglich sein soll.

Ein Exekutivkomitee, welches am 23. September 1904

zu St. Louis eingesetzt wurde, besteht aus den Herren Svante Arrhenius, George Hale und Arthur Schuster. Dieses hat eine Versammlung von Vertretern des Unternehmens für den 27. September dieses Jahres nach Oxford geladen. Die K. Akademie hat ein Spezialkomitee, bestehend aus den Herren Boltzmann, F. Exner, Hann, v. Lang und Weiss, eingesetzt und hat beschlossen, den Direktor der Sternwarte Hofrat Weiss nach Oxford zu entsenden.

Wir erlauben uns, dies den assoziierten Akademien zur gefälligen Kenntnis zu bringen und das Unternehmen der Sonnenforschung selbst, obwohl ausserhalb der Liste der Unternehmungen der Assoziation stehend, im Sinne des zu St. Louis gefassten Beschlusses der wohlwollenden Förderung zu empfehlen.

Das Präsidium der Kaiserl. Akademie der Wissenschaften.

SUESS. v. LANG.

No. 20.

SUGGESTIONS REGARDING THE CONSTITUTION OF INTERNATIONAL UNION FOR CO-OPERATION IN SOLAR RESEARCH.[1]

I. Constitution of the Union.

1. The Union shall consist of the International Association of Academies and of Scientific Bodies interested in the progress of Solar Research. Each constituent body shall act through a Committee nominated for the purpose. Each member of such Committee shall be considered a member of the Union.

2. An International Committee shall be organised through the election of one member by each constituent body.

[1] The articles of constitution as amended and finally adopted by the Oxford Conference are printed under No. 35, p. 242.

The President and the Vice-Presidents of the Union shall be *ex officio* members of the International Committee. The International Committee may further add to its numbers by co-opting not more than three further members, who need not be members of any of the constituent bodies of the Union.

3. There shall be an Executive Committee consisting of three members of the International Committee. Two of the three shall be elected by the International Committee at one of its meetings, and shall hold office until the following meeting. The third member of the Executive Committee shall be nominated by the leading Academy of the International Association of Academies, and shall hold office only while that Academy continues to act as leading Academy.

II. Meetings of the Union.

4. There shall, as a rule, be a triennial assembly of the Union for the purpose of discussing

(*a*) Questions of administration and organisation.

(*b*) Scientific questions relating to Solar Physics.

The meetings for the discussion of questions of administration and organisation shall be confined to members of the International Committee, and shall be referred to as meetings of the International Committee.

Scientific questions shall be discussed at meetings in which all members of the Union may take part; such meetings shall be referred to as meetings of the Union.

The assembly of the Union shall consist partly of meetings of the Union, and, if necessary, partly of meetings of the International Committee.

Each constituent body may send as many delegates to the meetings of the Union as wish to take part in its proceedings, but each body shall only have one vote, which

shall be cast (unless the society specially nominates another of its members) by the representative of the society on the International Committee.

5. The Executive Committee may invite any person interested in questions of Solar Physics, but not belonging to any constituent body, to take part in the meetings of the Union. Such invited guests shall have no vote, but shall have the right to take part in all scientific discussions.

6. In case the Executive Committee is of opinion that there is not sufficient matter of interest for discussion, they may communicate with the constituent bodies, who shall then decide by a vote, taken by correspondence, whether the triennial meeting shall be postponed to another year, provided that no such postponement shall take place unless a majority of two-thirds is obtained.

III. Procedure for Discussion of Scientific Questions.

7. Any of the constituent bodies of the Union, or any member of the Union, may send a proposal to the Executive Committee to include a particular question of the Solar Physics among the problems to be dealt with by the Union. If the proposal comes from a society, and if the question is included among the general subjects of investigation approved by the International Committee, the Executive Committee shall act as provided for in Articles 9 and 10. If the proposal comes from a member of the Union, the Executive Committee shall have power to act or not according to its discretion.

8. A subject may also be included amongst those to be dealt with, by a resolution of a meeting of the Union.

9. When a subject is included amongst those to be dealt with by the Union, the Executive Committee shall inform each of the constituent societies of the fact and ask it to

express an opinion on the subject, either collectively or through any of its members. The Executive Committee may also ask the advice of any worker specially interested in the subject, though he may not be a member of the Union.

10. The Executive Committee may then make further additions, and shall circulate the completed report among the constituent societies.

11. Any society, or any member of it, may also, on its own behalf, approach any other society, or member of it, and may prepare a report on any subject without being invited to do so by the Executive Committee. But in this case its report shall only be published on a resolution of the Union at its next meeting.

12. No decision shall be arrived at on any matter on which unanimity does not exist, except through a vote of the Union or of the International Committee at one of its meetings.

13. Nothing in the above shall be interpreted as interfering with the full liberty of any society or any one of its members to act independently or to prosecute any research on its own behalf.

IV. Limitation of Subject.

14. The subjects to be dealt with by the Union shall refer to:—

(1) Organisation of observations and their methods of reduction.

(2) The conduct of laboratory experiments likely to clear up Solar problems.

The Union as such shall not undertake the discussion of the relative merits of rival theories, though it may organise observations or experiments likely to throw light on such theories.

V. Place of Meeting.

15. At each of the triennial meetings of the International Committee, the place of the subsequent meeting shall be determined. The date of the meeting shall either be fixed at the same time or left to the decision of the Executive Committee, who shall in that case consult, by correspondence, the members of the International Committee.

VI. Officers of the Union.

16. There shall be a President and two Vice-Presidents of the Union. The President and Vice-Presidents shall be elected at the close of each of the Triennial Meetings of the International Committee. They shall begin their term of office at the conclusion of the triennial meeting at which they are elected, and hold office until their successors are appointed.

There shall be a Chairman of the Executive Committee, who shall be responsible for the interchange of correspondence in the intervals between the meetings, and for the publication of any matter the printing of which shall be decided upon either by the International Committee or by the Executive Committee.

Each constituent society shall be at liberty to correspond directly with any other society on matters connected with the work of the Union, but it is desirable that copies of such correspondence, if affecting the work of the Union, shall be communicated to the Chairman of the Executive Committee.

There shall be a Central Bureau (not necessarily of a permanent character) which shall be under the direction of the Chairman of the Executive Committee. The work of the Central Bureau shall be the conduct of the correspondence and publications of the Union.

There shall be a Computing Bureau in charge of one of the members of the International Committee. The work of the Computing Bureau shall be the conduct of such matters concerning calculations and reductions of observations as may be decided upon by the Executive Committee. The International or Executive Committee may, however, charge any other institution or member of the Union with the discussion and reductions of observations dealing with special portions of the subject. The Director of the Computing Bureau shall have power to initiate and carry out in the Bureau any calculations or reductions connected with the work of the Union, provided that the work dedided upon by the Executive Committee shall not thereby be delayed.

PART V.

Second Conference of the International Union for Co-operation in Solar Research, held at Oxford, September, 1905.

No. 21.

Minutes of Proceedings.

The following delegates attended the Conference:—

The names distinguished by a dagger (†) denote members of the International Committee.

Name.	*Representing.*
Prof. K. Ångstrom,†	Kongl. Vetenskaps Akademien, Stockholm.
Prof. A. Bélopolsky,†	Académie Impériale des Sciences, St. Petersburg.
Prof. W. W. Campbell,	National Academy of Sciences, Washington.
Sir W. Christie,	Royal Society, London.
Fr. Cortie,	Royal Astronomical Society, London.
Cte. de la Baume Pluvinel,	Société Astronomique de France, Paris.
Mr. H. Deslandres,†	Société Astronomique de France, Paris.
Mr. Fabry,	Sociéte de Physique, Paris.
Prof. A. Fowler,	Royal Astronomical Society, London.
Mr. G. E. Hale,†	National Academy of Sciences, Washington.
Major E. H. Hills,	Royal Astronomical Society, London.
Mr. J. Janssen,	Académic des Sciences, Paris.
Prof. W. H. Julius,†	Koninktigke Akademie van Wetenschappen, Amsterdam.
Prof. H. Kayser,†	Deutsche Physikalische Gessellschaft, Berlin.

Sir J. N. Lockyer, Royal Society, London.
Dr. W. J. Lockyer,† Solar Commission of International Meteorological Committee.
Mr. H. F. Newall, Royal Society, London.
Mr. Pérot,† Sociéte de Physique, Paris.
Prof. A. Schuster,† Royal Society, London.
Prof. H. H. Turner, Royal Astronomical Society, London.
Prof. E. Weiss, International Association of Academies.
Mr. W. E. Wilson, Royal Ast. Soc. (London).

The following also attended the meetings of the Conference: —

Messrs. F. A. Bellamy and H. C. Plummer, acting as Secretaries; and

Mme. Ångström, Father Cirera (representing the Geographical Institute,* Madrid), Messrs. G. Duffield and F. W. Dyson, Prof. W. S. Eichelberger, Father Froc (Zikawei Observatory), Dr. J. Halm, Professor G. F. Hull, Mme. Janssen, Mdlle. Janssen, Prof. Littell, Mr. G. Millochau, Dr. W. N. Shaw, Messrs. Stefanik and H. White, and Prof. Wolfer.

*The observatories of Spain are under the direction of this Institute.

The first meeting was held on Wednesday, September 27th, at 10 a.m. There were present: —

K. Ångström, A. Belopolsky, W. W. Campbell, Sir W. Christie, Fr. Cirera, Fr. Cortie, Cte. de la Baume Pluvinel, H. Deslandres, G. Duffield, F. W. Dyson, Mr. Fabry, A. Fowler, G. E. Hale, J. Halm, E. H. Hills, J. Janssen, W. H. Julius, H. Kayser, Sir J. N. Lockyer, W. J. Lockyer, M. Milléchau, H. F. Newall, A. Pérot,

A. Schuster, M. Stefanik, H. H. Turner, E. Weiss, W. E. Wilson, H. White, Prof. Wolfer, Mme. Janssen, Mdlle. Janssen, F. A. Bellamy, and H. C. Plummer.

Professor H. H. Turner took the chair, and briefly referred to the meeting held at St. Louis, U.S.A., in September, 1904, where it had been decided to hold the second meeting at either Meudon or Oxford. He explained the circumstances under which Oxford had been selected by the Executive Committee.

The Sub-Warden of New College (Mr. P. E. Matheson), in the unavoidable absence of the Warden (Rev. Canon Spooner), gave a hearty welcome to the Conference, and expressed his belief that international meetings of this kind tend to promote peace among the nations.

Professor Schuster, as Chairman of the Executive Committee, thanked the Sub-Warden and Fellows of New College for coming to the assistance of the Union at a critical period by offering their hospitality, which was gratefully accepted. He said, their first duty was to elect a chairman for each day. The Executive Committee suggested that, as a general principle, the chair should be taken by a representative of the country in which the Conference was held. He therefore proposed that Sir W. H. M. Christie, the Astronomer Royal, be chairman on the first day (Wednesday), and Professor Turner should take the chair on Thursday. The proposal was seconded by Sir Norman Lockyer, and carried.

Sir N. Lockyer proposed, and Professor Schuster seconded, that M. Janssen be Honorary President of the Conference. The proposal was carried unanimously.

The following provisional programme, which had been drawn up by the Executive Committee, was then put to the meeting and approved:—

Wednesday, September 27th.

10 a.m. 1. Introductory Proceedings.
2. Election of Officers for the Meeting.
3. Approval of Programme.

10.45 a.m. 4. Discussion on Standards of Wave-length.
5. Discussion of Co-operation in Spectro-heliograph Work, including related Spectroscopic Investigations.

Thursday, September 28th.

10 a.m. 1. Discussion of Co-operation in Measurements of the Intensity of Solar Radiation.
2. Discussion of the Spectroscopic Determination of Solar Rotation.
3. Discussion of other matters brought forward by Delegates, as far as time permits.

2.30 p.m. Further discussion of the Constitution of the Union, if necessary.

Friday, September 29th.

10 a.m. 1. Place and date of next Meeting.
2. Election of Officers.
3. Appointment of Committees.
4. Formal Resolutions.

1 p.m. Adjournment of the Conference.

The Chairman then suggested, and it was resolved, that the method of voting indicated in the proposed Constitution whereby each Society had one vote, should be provisionally adopted.

Resolved, that Mr. F. A. Bellamy and Mr. H. C. Plummer, from the University Observatory, Oxford, act as Secretaries.

At 10.40 a.m. the meeting adjourned till 11 a.m.

At 11 a.m. the chair was taken by the Astronomer Royal (Sir W. H. M. Christie), who called upon Professor Kayser to open the discussion on "Standards of Wavelength."

Professor Kayser spoke as follows: Die gedruckt vertheilten Aeusserungen zahlreicher Forscher zeigen eine so erfreuliche Uebereinstimmung über die Wege, die zur Erlangung besserer Wellenlängen-Normalen führen sollen, dass ich mich auf wenige Worte zur Einführung der vorliegenden Resolutionen beschränken zu sollen glaube.

Als an Stelle der Wellenlängen von Ångström die von Rowland traten, glaubte man allgemein ein System zu besitzen, welches nahezu frei von absoluten und relativen Fehlern sei. Aber sehr bald zeigten die Messungen der Cadmium Linien durch Michelson, dass Rowland's System einen erheblichen absoluten Fehler habe.

Wir sind ferner alle einig, dass es auch relativ genommen periodische und individuelle Fehler besitzt, von denen man sich nicht befreien kann, ohne alle Linien neu zu messen. Für den absoluten Werth müssen wir von irgend einer Linie ausgehen, und es liegt nahe, die rothe Cd-Linie von Michelson und Benoît zu nehmen, weil sie die einzige ist, welche bisher genügend genau bestimmt ist und weil sie auch allen billigen Forderungen entspricht. Doch mag diese Frage vorläufig offen bleiben. Alle übrigen Linien können nur relativ zu dieser Normalen erster Klasse gemessen werden, was sich mit Hülfe der Interferenzmethoden, namentlich mit dem Instrumente von Fabry und Pérot, ausführen lässt, verhältnissmässig leicht im sichtbaren Spectrum, schwie-

riger wahrscheinlich im Ultraviolett; doch sind auch hierfür bereits Methoden gefunden. Um die Arbeit nicht zu verzögern, wird man jedenfalls vorläufig diese Messungen auf die rothe Cd-Linie basiren müssen. Diese Normalen zweiter Klasse dürfen nicht weiter als 50 Å. von einander entfernt liegen, weil man sonst nicht mit der erforderlichen Genauigkeit zwischen ihnen interpoliren kann. Durch Interpolation an Gitteraufnahmen sollen nämlich die Normalen dritter Ordnung in Abständen von je 5 bis 10 Å. ermittelt werden. Es lässt sich hoffen, dass auf diese Weise eine genügende Anzahl von Normalen mit einer Genauigkeit von 1 bis 2 Tausendstel Å. erhalten wird. Alle Messungen sollen von mindestens zwei unabhängigen Beobachtern gemacht sein, um die Richtigkeit der Zahlen zu garantiren. Bei der Wahl der Lichtquelle ist der Funke auszuschliessen, weil die Funkenlinien im Allgemeinen weit unschärfer sind, als die Bogenlinien, weil die Funkenspectren viel lichtschwächer sind, vor allem auch, weil noch keine Sicherheit da ist, dass die Wellenlängen der Funkenlinien unveränderlich sind. Da die Veränderlichkeit der Intensität bei ihnen so gross ist und die Erregungsvorgänge sehr complicirt, so ist a priori eine kleine Veränderlichkeit der Wellenlänge nicht ausgeschlossen; sie ist bekanntlich mehrfach behauptet, eben so oft bestritten worden. Beim Bogen kennen wir den Einfluss des Druckes auf die Wellenlänge; die Aenderungen sind aber so klein, dass Aenderungen des Luftdruckes nicht in Betracht kommen. Der Bogen unter Atmosphärendruck bietet also Gewähr für Constanz der Wellenlänge. Es wird sich aber empfehlen, die Stromstärke in mässigen Grenzen zu halten, für Eisen 6 bis 10 Amp.; denn wir wissen noch nicht, ob nicht bei ungewöhnlichen Verhältnissen doch kleine Aenderungen eintreten. Was das zu wählende Metall

betrifft, so empfiehlt sich Eisen aus vielen Gründen: wegen seiner starken Verbreitung als Verunreinigung, welche z. B. in jedem Kohlebogen Eisenlinien in grosser Anzahl erscheinen lässt; wegen des guten Brennens des Eisenbogens, wegen des Reichthums des Spectrums an Linien und characteristischen Gruppen, wegen der Schärfe und Intensität vieler Linien. Nicht für alle Theile des Spectrums wird sich Eisen als ausreichend erweisen. Dann wird Titan sehr geignet sein, doch wird man im Nothfall für die Normalen zweiter und dritter Klasse auch andere Elemente heranziehen können. Ich glaube damit alles angeführt zu haben, was zur Begründung der Ihnen vorliegenden Resolutionen dienen kann.

Professor Kayser then formally moved the following four resolutions:—

1. The wave-length of a suitable spectroscopic line shall be taken as the primary standard of wave-length.*

2. Secondary standards are required at distances not greater than 50 Ångström units. These secondary standards should be referred to the primary standard by means of an interferometer method. The source of light should be obtained by means of an electric arc of from 6 to 10 Ampères.

3. A Committee shall be appointed to select the standards and to organize the determination of their wave-lengths in terms of the primary standard in at least two independent laboratories.

* At the meeting of Friday an addition was made to this resolution.

4. The same Committee shall be charged with the selection of tertiary standards which shall be at distances of from 5 to 10 Å. The wave-lengths of these tertiary standards are to be obtained by interpolation with the help of gratings.

Mr. Pérot made the following communications:—

Je commence par déclarer tout d'abord que je suis d'accord en tous points avec M. Kayser et que M. Fabry partage également les vues que ce savant vient de nous exposer. Vous me permettrez toutefois de revenir sur un point et d'en développer quelques autres.

Tout d'abord on ne peut songer à déterminer les longueurs d'onde des differentes radiations relativement au mètre, c'est à dire en valeur absolue comme l'ont fait MM. Michelson et Benoît pour une radiation, la raie rouge du Cadmium. Il faut donc mesurer le rapport des longueurs d'onde relativement à celle d'une radiation choisie comme fondamentale, et comme dans les mesures spectroscopiques courantes tout se fait par interpolation, on devra choisir un nombre convenable d'étalons convenablement répartis dans l'étendue du spectre. On est ainsi amené à:—

1°—Choisir un étalon fondamental.

2°—Choisir des étalons secondaires dont chacun sera mesuré directement relativement à l'étalon fondamental; il paraît souhaitable que la distance entre deux étalons consécutifs soit voisine de 50 Ångström.

3°—Des étalons tertiaires mesurés par interpolation relativement aux étalons secondaires.

Choix de l'étalon fondamental.—La radiation doit être simple, invariable et facile à reproduire, la raie doit être

aussi fine que possible pour que sa longueur d'onde soit parfaitement définie. Aucune radiation ne satisfait à ces conditions aussi bien que la raie rouge du Cadmium; on a fait, il est vrai, à son choix, l'objection que sa longueur d'onde était trop grande et qu'elle ne se prêtait pas à l'emploi des procédés photographiques; il est certain que s'il était possible de trouver un étalon fondamental dans la partie verte du spectre il y aurait sans doute lieu de le choisir, et M. Michelson l'aurait choisi, mais on n'en connaît pas; la difficulté signalée plus haut n'intervient d'ailleurs que dans la mesure des étalons secondaires et pourra vraisemblablement être surmontée; dans les opérations photographiques de spectroscopie ordinaire on n'a jamais besoin de l'étalon fondamental; il sera de lui comme du prototype international du mètre qui ne sert jamais dans les mesures habituelles, et auquel cependant sont rapportés tous les étalons métriques.

Etalons secondaires.—Il est vraisemblable que le spectre du fer ou celui du titane fourniront des étalons secondaires convenablement répartis, ces spectres seront donnés par l'arc électrique où les longueurs d'onde sont presque invariables, tant pour le spectre visible que pour le spectre ultra-violet.

Etalons tertiaires.—Ces étalons pourront être choisis dans un spectre quelconque et ne seront pas soumis aux mêmes conditions de finesse que les étalons secondaires.

Choix de l'unité de longueur.—Ce qui précède implique des mesures de rapport et est indépendant des mesures absolues; en spectroscopie d'ailleurs on ne fait que des mesures relatives; il importe seulement que le nombre qui représente chaque radiation soit fixé, et à cet effet il suffit de déterminer une fois pour toutes la valeur attribuée à la longueur d'onde de l'étalon fondamental. On pourrait

donc attribuer à cette longueur d'onde la valeur la plus simple, c'est à dire 1; cette solution a d'ailleurs été proposée, mais elle a été rejetée, d'abord parce qu'elle bouleverserait les habitudes reçues et ensuite parce qu'elle compliquerait les calculs métrologiques.

On est alors conduit à exprimer le mieux possible en unité métrique la longueur d'onde fondamentale; nous ne proposons pas toutefois de choisir une fraction décimale du mètre comme unité de longueur d'onde, mais seulement de fixer une fois pour toutes pour cette longueur d'onde un nombre invariable se rapprochant le plus possible de la valeur métrique; on constituerait ainsi une unité de longueur parfaitement définie et invariable, très voisine de l'unité métrique, qui recevrait un nom spécial, celui d'Ångström par exemple. Du moment que l'on est décidé à toucher aux longueurs d'onde données par Rowland, il ne semble pas y avoir un intérêt majeur à réduire les changements au minimum et il vaut mieux faire abstraction de ces nombres. Il résulte bien clairement de ce qui précède que l'unité une fois choisie devra rester fixe et ne pas être soumise aux progrès de la Métrologie, comme paraît le redouter M. Hartmann.

Le choix du nombre représentant la longueur d'onde de l'étalon fondamental peut être remis à une date ultérieure. On possède actuellement les seuls résultats des expériences de MM. Michelson et Benoît, et quoique l'on doive avoir toute confiance dans les nombres résultant de ces mesures, il est désirable de les confirmer par l'emploi d'autres méthodes; c'est l'avis du Comité international des Poids et Mesures qui, considérant l'utilité de cette vérification, a chargé le Bureau International de procèder de concert avec M. Fabry et moi-même à une série d'expériences faites d'après une nouvelle méthode; ces expériences sont en cours d'exécution; on pourrait toutefois

admettre pour le moment le nombre de MM. Michelson et Benoît.

J'ai donc l'honneur de vous proposer d'ajouter au paragraphe 1 des résolutions proposées par M. Kayser les phrases suivantes :—

> On fixera une fois pour toutes le nombre qui représente cette longueur d'onde; il fixera dès lors l'unité de longueur d'onde qui devra différer aussi peu que possible de 10^{-10} mètre et s'appellera Ångström.

M. Fabry expressed his concurrence.

M. Deslandres then expressed his opinion that some of the lines found in the heads of nitrogen bands were suitable as standards of wave-length, because they were sharper than the iron lines and unaffected by pressure or the magnetic field. He proposed to include such lines among the secondary standards. The Chairman suggested that additions, if any, might most conveniently be made by the Committee which it was proposed to appoint. This was accepted by M. Deslandres. The four resolutions were then put to the meeting and carried.

The Conference proceeded to the Discussion of Co-operation in Spectroheliograph work, including related spectroscopic investigations." Professor G. E. Hale, in opening the discussion, quoted as an instance where co-operation had already been successfully organised, the measurement, at one centre, of the 8-inch solar photographs taken at different observatories—Greenwich, Dehra Dûn, and Mauritius. He regarded the work carried out at the Royal Observatory, Greenwich, in this connexion, as sufficient for the general study of the statistics of spots. But there was need for supplementary study of the details

of the spots with the spectroheliograph and spectroscope. Investigations with these instruments should not be confined to the spots, but it was necessary to include much larger areas in which the calcium radiations played an important part. He pointed out that these regions could be observed when spots were absent. He recommended that work should be continued with the instruments at present in use. It was true that these instruments were very different in dispersive powers, and also in other respects, but the photographs were comparable. At present it would not therefore be necessary to disturb existing installations by laying too great a stress on uniformity. He thought the Solar Union could act in perfect harmony with the Solar Commission of the International Meteorological Committee, which dealt with the meteorological aspect of the question. He moved that a preliminary committee be appointed to draw up definite resolutions with a view to the appointment of a standing committee or committees. He suggested the following five subjects as falling within the purview of this committee.

(*a*) Direct solar photographs of 8-in. diameter.

(*b*) Solar photographs enlarged (on Janssen's scale).

(*c*) Photographs made with the spectroheliograph.

(*d*) Photographs of the H and K lines on a large scale.

(*e*) The spectra and radiations of sun-spots.

M. Deslandres made the following communication : —

J'ai accepté avec empressement l'idee du congrés, ayant reclamé depuis 1893 l'enregistrement continu de tous les éléments variables du Soleil, et la coopération de tous les observatoires solaires. A mon avis il faut d'abord examiner dans leur ensemble toutes les observations solaires (observations de la surface avec la simple lunette, observa-

tions de l'atmosphère avec le spectroheliographe et autres spectrographes enregistreurs) et les rendre aussi comparables que possible. J'émets le voeu que toutes ces images de la surface et de l'atmosphère aient autant que possible des diamètres qui soient égaux ou dans un rapport simple, qui soient par example des multiples de 10 cms. cette règle étant applicable si non aux épreuves originales, du moins aux reproductions et publications. J'émets aussi le voeu que les publications soient faites par rotations entières du Soleil, surtout si l'on peut s'entendre sur le choix, au moins provisoire, de la durée moyenne de rotation des taches.

Les observations de la surface sont anciennes et organisées d'une manière remarquable, étant centralisées déjà pour la statisque des taches à Greenwich et à Zurich. Les épreuves réunies a Greenwich, larges de 0m. 20. ont des dimensions conformes au voeu précédent, de même les épreuves de Meudon, destinées a l'étude de la granulation, et larges de 0m. 30. Mais, à ma connaissance, on n'a pas encore organisé l'enregistrement continu (si utile à certains moments) de la surface ou de portions intéressantes de la surface. Je propose:

(*a*) Des enregistreurs automatiques de la surface, susceptibles de fournir, à un moment donné, des épreuves, aussi rapprochées que possible, de la surface ou de portions de la surface—Diamètre de l'image, au moins 0m. 20. Les observations de l'atmosphere ou, pour préciser, de la chromosphère sont encore en voie d'organisation. Pour être complètes, elles devront donner, (1) les formes ou les intensités, (2) les mouvements radiaux, (3) la composition chimique de la chromosphère, de ses diverses vapeurs et de leurs couches successives superposées. Ce résultat sera obtenu avec des spectrographes enregistreurs, que, pour simplifier, j'appelle spectro-enregistreurs, et qui, au moins pour l'étude des formes, isolent soit les raies brillantes

H_2 ou K_2, soit les raies noires H_1 K_1, H_3 K_3 soit une quelconque des très nombreuses raies noires du spectre normal. Mais les raies H et K, à cause de la séparation nette de leurs trois composantes, à cause de leur sensibilité extraordinaire aux variations de pression et d'excitation électrique, sont de beaucoup les plus utiles. Aussi je propose : —

(*b*) Des spectro-enregistreurs des formes ou spectroheliographes, susceptibles de donner les images de H_1 ou K_1, H_2 ou K_2, H_3 ou K_3, qui correspondent aux chromosphères casse, moyenne et supérieure. Diamètre de l'image : 20 cms.

Plusieurs appareils capables de donner les images H_1 K_1, H_2 K_2, sont en service ; ils ont des dispersions differentes ; ce qui entraine dans les images de petites differences, qui doivent être signalées, mais ne peuvent entraver sérieusement la coopération.

Les images H_3 K_3 qui seront utilement comparées aux images similaires avec les raies de l'hydrogène, ne peuvent donner la couche correspondante bien separée des autres couches, à cause des grandes variations en largeur des raies H_3 et K_3.

(*c*) Des spectro-enregistreurs des vitesses (ou par sections successives) qui séparent nettement les couches H_2 H_3 K_2 K_3 donnent une idée de leurs épaisseurs, et enregistrent leurs vitesses radiales. (Diamètre de l'image le même que précédemment : 20 cms.) Ils sont nécessaires, pour l'étude de la formation des taches, de leur évolution, de leur action présumée sur les aimants terrestres. Ils se prêtent à un enregistrement sans arrêt, et ont l'avantage de donner en même temps les formes générales des vapeurs.

(*d*) Des spectro-enregistreurs proprement dits, ou enregistreurs de spectres qui juxtaposent automatiquement les spectres étendus de sections successives equidistantes

faites dans les taches et facules, c'est a dire dans les seules parties qui offrent en général des variations spectrales.

(*e*) Des spectro-enregistreurs des formes ou spectro-heliographs de grande dispersion pour les images des raies noires ordinaires et destinés a l'étude de la répartition des diverses vapeurs dans la chromosphère.

Ils peuvent donnent les images H_3 et K_3, les spectro-heliographes precédent du paragraphe (*b*) donnant alors seulement H_1 K_1, H_2 K_2 et étant alors de dispersion faible. Les enregistreurs des formes, des vitesses et des spectres sont déjàrealisés a Meudon, au moins sous une forme réduite.

La coopération sera particulièrement facile avec les spectro-enregistreurs des vitesses.

Mr. H. F. Newall seconded the appointment of a preliminary committee, and was followed by Prof. Bélopolsky, Sir J. N. Lockyer, M. Deslandres, Fr. Cortie, Dr. W. J. Lockyer, and Prof. Fowler. Sir J. N. Lockyer spoke of the importance of continuing individual work in addition to the co-operative work, and thought the individual work need have no fear of being hampered or in any way interfered with by co-operation.

The Chairman then put Professor Hale's proposal, which was carried by the meeting. It was further

> Resolved: "That the Committee shall consist of Prof. Bélopolski, M. Deslandres, Prof. Hale, Sir J. N. Lockyer and Dr. Wolfer," and that the Committee be requested to report to the Conference on the following day.

M. Deslandres handed in two communications* bearing the titles:—

*The communications are reprinted on pages 184 and 193 of these *Transactions*.

(1) Remarques sur l'état actuel des Recherches solaires solaires et sur les moyens de les améliorer.

(2) Sur le sens précis de mots anciens et sur le choix de mots nouveaux.

The meeting adjourned at 0-45 p.m.

Wednesday, September 27, 1905.—Afternoon meeting.

The afternoon meeting for discussion of the proposed Constitution of the Union was held at 2-30 p.m. Professor Schuster, as Chairman of the Executive Committee, took the chair.

Present: Messrs. Bélopolsky, Campbell, Sir W. Christie, Cirera, Cortie, De la Baume Pluvinel, Deslandres, Duffield, Dyson, Fabry, Fowler, Froc, Hale, Halm, Hills, Janssen, Kayser, Sir J. N. Lockyer, W. J. Lockyer, Millochau, Newall, Pérot, Stefanik, Turner, Weiss, Wilson, Wolfer, F. A. Bellamy and H. C. Plummer as secretaries.

Agreed that the Solar Physics Committee of the British Board of Education be added to the original list of constituent bodies.

The meeting proceeded to discuss the suggested Articles of the Constitution of the Union as circulated by the Executive Committee (see page 141).

Resolved: In Section I., article 1, that the words "of the International Association of Academies and" be omitted for the present, but be inserted when the International Association of Academies have intimated their agreement.

Sir J. Norman Lockyer proposed to insert in the same article the words "and institutions" after "scientific bodies." After a discussion, in which Messrs. Campbell,

Deslandres, Newall, and Turner took part, the further consideration of the proposal was deferred.

Resolved: To add to Section I., article 1, at the end, the words:

"There may also be corresponding members who shall be members of the Union."

Resolved:

In I., 2, omit the paragraph beginning "The President and Vice-Presidents."

In I., 3, omit the last sentence until the International Association of Academies have expressed their agreement.

In II., 4, first line: omit "as a rule" and substitute "biennial assemblies" for "triennial assembly."

Insert II., 6, in II., 4, substituting "biennial" for "triennial."

During the discussion of II., 5, M. Deslandres moved, and Father Cirera seconded, a proposal that the corresponding members should have votes at the meetings of the Union. It was pointed out that this would place the corresponding members in a more advantageous position than the members of constituting societies, each society only having one collective vote. The motion was not pressed on the understanding that the question could be raised again at a subsequent meeting.

Resolved: For III., 11 and 12, to read as follows:

11. The Executive Committee shall have power to receive reports which may be drawn up by any of the Constituent bodies or by any member of the Union; but such reports, not being invited by the Executive Committee, shall only be published by a resolution of the Union at its next meeting.

12. No important resolution shall be adopted as a resolution of the Union except through a vote of the Union or of the International Committee at one of their meetings. But this regulation shall not be held to prevent the Union from the deputing of specific powers of decision to a Committee appointed at a meeting of the Union.

Resolved: In IV., 14, under (2) to substitute "bearing on" for "likely to clear up."

Resolved: In V., 15, in the first sentence omit "triennial" and add at the end: "and the chairman who is to preside at that meeting shall be elected."

During the discussion of Section VI., Major Hills asked who would supply the funds for the maintenance of the Central and Computing Bureau. Sir Norman Lockyer suggested that the question of the establishment of the Bureau might be deferred to a subsequent meeting. Father Cortie and Professor Hale did not wish the matter to be deferred. The latter referred to the offer made by the University Observatory, Oxford, through Professor Turner. The question of money was a serious one, but no serious difficulties would arise during the next few years. Professor Schuster explained the situation as far as Manchester was concerned. Sir Norman Lockyer stated that if it could be shown that any useful purpose could be served by the immediate establishment of these bureaus, he would support the proposal.

Resolved: That VI. shall read as follows:—

VI. Officers of the Union.

16. There shall be a Chairman of the Executive Committee elected by the International Committee,

who shall be responsible for the interchange of correspondence in the intervals between the meetings, and for the publication of any matter the printing of which shall be decided upon either by the International Committee or by the Executive Committee.

In the case of correspondence carried on between constituent bodies on matters connected with the work of the Union, it is desirable that copies of such correspondence, if affecting the work of the Union, shall be communicated to the Chairman of the Executive Committee.

17. There shall be a Central Bureau charged with the conduct of the correspondence and publications of the Union, and of such other duties as may be deputed to it by the Union (not necessarily of a permanent character). The Central Bureau shall be under the direction of the Chairman of the Executive Committee.

18. There shall be a Computing Bureau under the direction of one of the members of the International Committee. The work of the Computing Bureau shall be the conduct of such matters concerning calculations and reductions of observations as may be decided upon by the Executive Committee. The International or Executive Committee, may, however, charge any other institution or member of the Union with the discussion and reduction of observations dealing with special portions of the subject. The Director of the Computing Bureau shall have power to initiate and carry out in the Bureau any calculations or reductions connected with the work of

the Union, provided that the work decided upon by the Executive Committee shall not thereby be delayed.

Resolved: To add at the end of the Articles of Constitution:—

VII. Amendments.

19. The above regulations may be added to or amended by a vote of the Union at one of its meetings provided that notice of the proposed amendment or addition has been given at least three months before the meeting.

The Articles of the Constitution as amended were then put to the meeting as a whole and passed unanimously. (The articles will be found reprinted in English, French and German on pages 242, 247 and 252.)

Meeting, Thursday, September 28th, 1905.

The chair was taken by Professor H. H. Turner, at 10-10 a.m. Present: Messrs. Ångström, Bélopolsky, Campbell, Christie, Cirera, Cortie, De la Baume Pluvinel, Deslandres, Duffield, Eichelberger, Fabry, Fowler, Froc, Hale, Halm, Hills, Hull, Janssen, Julius, Kayser, Littell, J. N. Lockyer, W. J. Lockyer, Millochau, Newall, Pérot, Schuster, Shaw, Stefanik, Weiss, Wilson, Wolfer, Mme. Ångström and Mme. Janssen, also Messrs. Bellamy, Dyson and Plummer (secretaries).

Professor Schuster opened the discussion "On Co-operation in measurements of the intensity of Solar Radiation." After briefly introducing the subject and welcoming the presence of Professor Ångström, he

formally moved the five resolutions which had been printed and distributed to the Delegates.

The chairman suggested that Prof. Ångstrom should first communicate the note he had written upon these questions, and the vote on Professor Schuster's resolutions could then be taken seriatim.

Professor Ångström gave a brief account of his paper "On the methods for the investigation of the changes in solar radiation." The paper, as well as a second one by the same author "On the compensating pyrheliometer," was handed to the chairman of the Executive Committee for publication in the *Transactions* of the Union (see pp. 178 and 180).

Professor Julius also made a short statement referring to the question of solar radiation. The meeting was informed that the statement would be printed in the *Transactions* of the Union, see p. 196).

The discussion was continued by Dr. Shaw, who thought that one of the objects the Union should have in view, was to extend the practice of observing solar radiation from the Laboratory to the Observatory. He referred to the International Commission appointed by the Meteorological Committee in 1896, and its reconstruction, as regarded this subject in 1905.

Sir W. Christie drew attention to the importance of measuring not only the average intensity of solar radiation over the whole disc, but of distinguishing between different portions such as the radiation of the parts surrounding a sunspot and that of the umbra of the spot. He referred to the work now in progress at Greenwich Observatory.

Prof. Hale, Mr. Wilson, and Prof. Bélopolsky took part in the discussion.

Professor Schuster's propositions were then placed before the meeting.

Resolved:

1. In order to secure uniformity it is desirable that observations on the intensity of solar radiation in different localities shall be made as far as possible with the same type of instrument.

2. That for the present Ångström's pyrheliometer be adopted as the standard instrument.

3. That it is desirable to obtain accurate comparisons between the records of Ångström's pyrheliometer and other standard instruments; and that Mr. Abbot, Professor Callendar, Professor Wladimir Michelson and Mr. W. E. Wilson be asked to assist the Union in this work.

Resolved: 4. That for the determination of the possible changes in the solar-radiation power, it is desirable to secure measurements of the intensity over limited ranges of the spectrum, which are not affected by absorption due to ozone, aqueous vapour, and carbonic acid.

Resolved: 5. That a Committee be appointed to draw up a scheme of co-operation, and proposals for the reduction of observations. That the Committee be requested to communicate the scheme and proposals to the Executive Committee with a view to initiating a system of observations according to the scheme.

It was proposed by Sir W. Christie, seconded by Prof. Hale, and resolved to add to the above resolutions.

6. "That the Union recognises the great importance of measurements by photography* as well as

* (On Friday morning, at Sir W. Christie's request, and with the permission of the meeting, the word "direct" was inserted before "photography."

by other methods, of the relative intensities of radiation, emitted by different parts of the solar surface, and desires to include such measurements in the subjects to be dealt with by the Union."

The meeting then entered on the discussion of the proposals concerning co-operation in the study of the phenomena observed on the surface of the sun. Sir N. Lockyer, as Chairman of the Committee, appointed on Wednesday, September 27th, submitted five resolutions which had been printed and distributed to the members:—

M. Deslandres wished the report to be referred to the Committee for reconsideration and revision.

Sir N. Lockyer, in reply, stated that the Committee had performed the function imposed upon it by yesterday's meeting. They had given full consideration to the matter, and a reconsideration would not, in his opinion, serve any useful purpose.

Prof. Hale and Prof. Wolfer supported Sir N. Lockyer, and objected to the delay which would be caused by a reconsideration of the resolutions.

Major Hills thought the Committee had gone beyond its powers.

M. Deslandres then withdrew his opposition.

Resolution 1 was then put, and, after some remarks by Fr. Cirera, carried *nem. con.*

Resolution 1: Co-operation is desirable in the various branches of Solar Research, such as visual and photographic observations of the solar surface, visual observations of prominences, and observations of the solar atmosphere with spectroheliographs of various types.

Resolution 2: When an institution has collected and co-ordinated results from various sources, members of the Union shall be requested to place their observations at the disposal of the said institution.

After explanations offered by Sir N. Lockyer in answer to a question put by Fr. Cortie, Resolution 2 was carried *nem. con.*

Resolution 3: In the case of investigations which have not yet been thus collected and co-ordinated, a committee specially nominated by the Union shall be charged with the work of preparation and organisation.

Sir N. Lockyer, Sir W. Christie, Prof. Schuster, M. Deslandres, Prof. Hale, and Mr. Newall took part in the discussion of this resolution. On the motion of Sir W. Christie, it was resolved to substitute the words "special committees nominated" in place of "a committee specially nominated."

This amendment was put to the vote, and carried.

On the motion of Sir N. Lockyer, it was resolved to substitute the words "collection and co-ordination" for "preparation and organisation."

On the motion of Professor Schuster, it was resolved to add the following words at the end of Resolution 3:—

"That the committees be requested to communicate the scheme and proposals to the Executive Committee, with a view of initiating a system of observations according to the scheme."

Carried *nem. con.*

Resolution 4: It is proposed forthwith to organise such co-operation in two branches of research:

(*a*) The study of the spectra of sun-spots.

(*b*) The study of the records, by means of the H and K light, of phenomena of the solar atmosphere.

After a short discussion in which Major Hills, M. Deslandres, and Fr. Cirera took part, this was carried.*

> Resolution: 5. The committee lays special stress on the fact that, notwithstanding the obvious utility of co-operation in certain cases, individual initiative is the chief factor in a very large number. It is as much the duty of the Union to encourage original researches as to promote co-operation.

After remarks by Prof. Hale and Major Hills, and the substitution of the word "Union" for "Committee," this was carried *nem. con.*

Agreed that the appointment of one or more committees be made on Friday.

The "Discussion of the spectroscopic determination of Solar Rotation," which formed subject 2 on the Agenda was then opened by Professor Campbell, who said that the problem of Solar Rotation was one of the more promising methods of interpreting solar phenomena. Carrington, Duner, Wilsing, Bélopolsky and others had deducted empirical formulæ expressing the law of solar rotation, and all obtained different laws. It was entirely possible that all were correct, as they were, in the main, observing different phenomena and perhaps different strata in the solar atmosphere. Dunér's visual observations were based on two Fraunhoffer iron lines, and his results may not be capable of substantial improvement. Nevertheless, we should expect that other selected iron lines or lines of other elements would give each its own law, the extreme cases furnishing quite different laws. Photographic methods should prevail, because they would apply equal or

*See amendment to this resolution carried on Friday, September 29.

greater accuracy to a long range of spectrum including a large number of lines of a variety of elements. Spectroheliograms should also be utilized, in so many elements as possible, in order to develop the law for a considerable number of strata. It also seems essential that the observations should extend at least over a complete sun-spot cycle.

Dr. Halm described the apparatus and method employed by him at Edinburgh. On the motion of Prof. Campbell, seconded by Dr. Halm, it was resolved that

> "The Union recognises the importance of investigating the problem of solar rotation by spectroscopic methods, and desires to include this subject among those to be dealt with by the Union."

Permission of the meeting having been obtained, M Pérot and Prof. Kayser proposed and seconded that the following words be added to the first of the resolutions referring to the standards of wave-length as passed on Wednesday, September 27th.

> "The wave-length of this line shall be fixed permanently, and thereby define the unit in which all wave-lengths are to be measured. This unit shall differ as little as possible from 10^{-10} metres and be called the Ångström."

This was carried.

M. Deslandres drew attention to the fact that, at the meeting of Wednesday afternoon, a proposal to alter the first article in the Constitution so as to include institutions as well as societies had been postponed. He moved that this matter be now considered.

Professor Schuster suggested that the article should be left as it stands until experience had shown that a change was desirable. We should probably be better able to judge

at the next meeting, and he therefore proposed that the question be deferred till then. This was agreed to by M. Deslandres.

The meeting adjourned at 0-30 p.m. until 10-30 a.m. on Friday, September 29.

Friday, September 29, 1905.

The chair was taken by Prof. Turner at 10-30 a.m. Present: Messrs. Ångström, Bélopolsky, Campbell, Cirera, Deslandres, Fabry, Froc, Hale, Julius, Kayser, W. J. Lockyer, Newall, De la Baume Pluvinel, Schuster, Weiss, Wolfer, and Bellamy, Hills and Plummer as secretaries. Also after 11-15, Messrs. Duffield, Hull, Janssen, Millochau and Stefanik.

Prof. Schuster moved two resolutions, notice of which he had given on Wednesday afternoon:—

"That the Union request the International Association of Academies to allow itself to be placed at the head of the list of bodies constituting the Union."

"That the International Association of Academies be asked to agree to nominate one of the three members of the Executive Committee. The Union suggest that the nomination of this member may conveniently be made by the leading Academy, the nomination holding while that Academy continues to act as leading Academy."

Carried *nem. con.*

The Constitution of the Union was then formally and unanimously ratified.

M. Deslandres, on behalf of M. Janssen and himself, proposed that the next Conference should meet at Meudon.

He explained the accommodation they were able to offer, and pointed out the advantages of a place of meeting in the neighbourhood of Paris.

The Union had already decided that the meetings should be biennial, and he therefore proposed the year 1907, and suggested either May or September as a convenient time.

It was resolved unanimously to hold the meeting at Meudon in 1907, but the great majority of those present expressed themselves in favour of holding the meeting in September. This was consequently agreed to.

Resolved: That the exact date of the meeting be determined by the Executive Committee.

It was mentioned that Dr. Arrhenius, who was not able to be present at the meeting, had expressed the desire to retire from the Executive Committee.

Mr. Newall proposed Professor Schuster as Chairman.

On the motion of Professor Newall, it was resolved:

"That Professors Schuster and Hale be the Executive Committee, and that Professor Schuster be the Chairman."

Mr. Newall proposed that the Central Bureau be at the Physical Laboratory of the University of Manchester. Carried *nem. con.*

Professor Schuster expressed his thanks for the honour in electing him Chairman, and felt sure that the University of Manchester would be gratified by this selection, and would assist the work of the Union as far as was in their power.

Professor Hale moved: "That the Computing Bureau be placed at the University of Oxford under the direction of Professor H. H. Turner."

This was seconded by Professor Weiss, supported by Professor Campbell, and carried unanimously.

Professor Turner expressed his willingness to undertake the work on behalf of the Union.

Appointment of Committees.

The following were then appointed members of the various Committees which had been instituted in the previous days:—

(*a*) Committee on Standards of Wave-length:
Professor Kayser (Chairman), Professor Ames, M. Fabry, Professor Michelson, and M. Pérot.

(*b*) Committee on Solar Radiation:
Professor Ångström (Chairman), Mr. Abbot, Professor Callendar, Dr. Julius, Professor Schuster, M. Violle, and Mr. W. E. Wilson.

(*c*) Committee on Co-operation in work with the Spectroheliograph:
Professor Hale (Chairman), M. Deslandres, Professor Frost, Dr. W. J. Lockyer, Dr. Riccò, and Mr. Michie Smith.

(*d*) Committee on Spectra of Sun-spots:
Professor C. A. Young[1] (Chairman), Professor Bélopolsky, Sir W. H. M. Christie, Fr. Cortie, M. Deslandres, Prof. Fowler, Sir N. Lockyer, Mr. Newall, and Dr. Wolfer.

Professor Schuster referred to the question of publication of the proceedings of the meeting, and of the papers which were handed in. He also wished to have instructions whether the resolutions were to be published in one language only, or in different languages.

On the motion of M. Pérot, seconded by Dr. Weiss, it was resolved that the resolutions and articles of the Union be published in English, French and German.

[1] Professor Young has found it necessary to resign the Chairmanship on account of ill-health, Professor Fowler acting as Secretary to the Committee.

M. Pérot and Dr. Kayser suggested that the resolutions and articles of constitution be printed and circulated as soon as possible. They could afterwards be embodied in the volume containing the papers.

Resolved: That where a resolution is printed in different languages, the language of the country in which the meeting, at which the resoltion was passed, was held, be taken as the authoritative text.

Mr. Newall pointed out an ambiguity in Resolution 4 (p. 170) referring to co-operation in the study of the solar surface. To clear up the ambiguity he proposed to insert the words "through the Union" after the word "organise" in this resolution. This was agreed to.

The Chairman stated that the Astronomer Royal, who had been obliged to leave the meeting, had expressed a wish to insert the word "direct" before "photography" in Resolution 6, referring to the measurement of solar radiation.

This was agreed to.

M. Pérot proposed that the cordial thanks of the Union be tendered to the Warden and Fellows of New College, Oxford, for the facilities afforded to the Union for their meeting, and for the hospitality of the College to foreign members of the Union. This was carried unanimously.

Professor Schuster proposed, that thanks be given to Messrs. Bellamy, Dyson, Hills and Plummer for their services as Secretaries during the Conference. This was passed by acclamation.

On the motion of Professor Hale, it was resolved that thanks be given to the University of Manchester for their timely financial assistance, which had rendered possible the printing of the various preliminary documents.

Fr. Cirera referred to the organisation of solar work, which he hoped would in future be carried out vigorously in Spain.

Prof. Hale proposed that steps should be taken through the Spanish Academy, to secure a representative of Spain in the Union.

Professor Schuster thought that the resolution should be made to apply to all countries which have not yet organised such work, and proposed that the following resolution be adopted:—

> "That in the opinion of the Conference, it would be desirable that countries, which do not contain scientific bodies forming part of the Union, should organise solar research by forming Committees, which could co-operate with the Union."

This was carried.

On the motion of Dr. Weiss, it was resolved that the thanks of the Conference be accorded to Sir W. Christie, Prof. Schuster, and Prof. Turner, who had acted as Chairmen during the meeting.

The Conference adjourned at 0-20 p.m. to September, 1907.

PART VI.

Papers Communicated to the Second Conference, held at Oxford, September, 1905.

No. 22.

The Compensating Pyrheliometer.

By Knut Ångström.

My compensating pyrheliometer is now an instrument that is very well known, I think, among the physicists of to-day; but, as I have the honour to show it at this meeting, for which the question concerning the best methods for measuring the radiation of the sun must be of the greatest importance, I have thought that a short explanation of the principles of the instrument, its management, and the improvements that it has undergone recently would be of some interest.

Two identical very thin metal bands, blackened on the outside, are suspended beside each other in a little frame fastened in a short tube, which, by means of two screws, can be directed towards the source of radiation to be studied. One of the bands can be protected against the radiation by means of a little screw attached to the opening of the tube. Through the other band we can send an electric current, and we can regulate this current by means of a resistance, so that the temperature will be the same in the two bands. Under these circumstances both bands will receive the same quantity of heat, and we can then estimate with very great accuracy the heat Q produced by the current, if we know the resistance (r) of the band and the strength of the current (i).

We have:

$$Q = \frac{60}{4{\cdot}19} \cdot \frac{ri^2}{b.a} = \text{Const. } i^2$$

(a = the absorbing power of the blackened surfaces; b = the width of the surfaces).

By this means we have accordingly a determination of the intensity of the radiation.

That the temperature is the same in the two bands can be determined by means of thermo-elements fastened on the back of the bands and connected to a galvanometer, that can be read by means of tube, mirror and scale in the ordinary well-known way. The strength of the current can be varied by means of a slide-rheostat, and the intensity of the current can be determined by a milli-amperemeter. It is easy to understand that by turning the screw and changing the current we can repeat the determinations as often as we wish.

The advantages, which this method for the determination of the radiant heat, offers in comparison with others for the same purpose are:—

1. That all corrections for the losses of heat by convection-currents, by conduction, etc., are perfectly eliminated, these losses of heat being the same for both bands.

2. That different instruments can be made without difficulty nearly identical, and that the constants can be determined with great accuracy.

3. That the instrument works rapidly; only about 30 secs. are necessary for one determination if the instrument is ready for use.

4. That the range over which the instruments can be used is very great. Intensities from

$$0{,}000025 \text{ to } 3 \frac{gr.\ cal.}{min.\ cm^2}$$

can be determined, which, of course, makes the instrument very proper for comparison of sources of very different intensities.

5. That the accuracy of the instrument is great. The greatest difference between determinations with different instruments—the constants being directly determined—will be 2 % and the difference between determinations of the same radiation with the same instrument only about $\frac{1}{4}$ %. Consequently this will be also the maximum difference between readings of instruments directly compared with one another.

The improvements that the instrument has undergone recently principally concern the material of the bands and the fastening of the thermo-elements. By the employment of manganin for the bands, all corrections for the influence of the temperature on the resistance of the bands are practically unnecessary. The greatest difficulty in the construction of the instrument was in the fastening of the thermo-elements on the bands; but latterly I have overcome it perfectly.

In brief, I think that the compensating pyrheliometer in its present form will fulfil most of the demands which can be made upon it, and I am sure it is the only instrument at present, rapid enough and of sufficient sensibility and accuracy for some of the investigations which now must be taken up concerning the radiation of the sun.

No. 23.

On the methods for the investigation of the changes in Solar Radiation.

By Knut Ångström.

There are many questions of great importance concerning the intensity of solar radiation. There is the

question of the distribution of solar radiation over the earth-surface; the question regarding the changes in solar radiation owing to the influence of the atmospheric absorption; and there is, finally, the question about the *temporary* variations, that the solar radiation will probably undergo *eo ipso*.

The first of these questions is mainly of meteorological interest; the second question—which primarily affects both the first and third—cannot be perfectly examined without elaborate physical laboratory researches concerning the absorption of the atmospheric gases. The last question—referring to the variations in the solar radiation itself—has, after the late remarkable work of Langley, become one of great interest, but is perhaps the most difficult of all. The solution of this question cannot probably be obtained without international co-operation, and I therefore take this opportunity to propose the ways which, according to my opinion, possibly may lead to satisfactory results.

The simplest method for these researches is certainly to be found in the simultaneous measurement of the total solar radiation at different stations. If, after having taken account of the different atmospheric influences, we find for different stations the same temporary changes in the solar radiation, that will evidently be a strong proof in favour of measurable variations in the radiation itself. Necessary conditions for this method are evidently—first, that all the measurements should be done with the same sort of instrument, or at least with instruments perfectly comparable with one another; and, secondly, that all the reductions in regard to the atmospheric conditions are made in accordance with identical principles.

A method that at first sight will offer many advantages over the one just mentioned in the spectrobolometric one,

and particularly the spectrobolometer used as a registering instrument. But it must be mentioned, first, that the ordinary law of absorption must be used with a certain reservation, the radiation falling on the spectrobolometric band not being strictly a simple one; secondly, that we always must base the calculation of the spectrobolometric measurements on simultaneous pyrheliometric observations; finally, the registering spectrobolometer is so expensive an instrument, and its use and the calculations of the results so troublesome, that the installation of such an instrument can be made only in exceptional cases, and must, I think, wholly be left to private initiative. Certainly, that instrument will be of greatest importance for the investigations of the atmospheric conditions, where it can be used. At present, I think, the instrument is in use only in America and Upsala.

But there is another method of investigating the different parts of the solar radiation, and that is, by means of absorbing substances placed before the opening of the pyrheliometer. Small water-vessels with plane-parallel sides have been used in the past for this purpose; but, we must remember that we can now return to this method with much greater advantage, because with the spectrobolometer we can make a careful investigation of the properties of the absorbing substances, and, of course, verify the nature of the transmitted radiation. With three pyrheliometric determinations, viz., of the total radiation, of the radiation passing through a cell of water, and, lastly, of the radiation passing through the water cell and a blue glass-plate (for example, No. 3,086 from Schott-Jena), we may determine the intensity—(1) of the total radiation, (2) of the extreme part of the infra-red radiation, which is most affected by the absorption of the water vapour, (3) of the blue and ultra-violet

part of the radiation (most affected by the dust-diffusion), and, lastly (4), of the radiation of the intermediate part of the spectrum (which is comparatively less affected by the influence of water and dust). We will then be able to decide the influence of water-vapour and dust, and if we find that the intensity of the intermediate part of the spectrum undergoes changes, which cannot be accounted for by the observed variations in the composition of the atmosphere, this will be a strong reason for supposing changes in the solar radiation itself.

I hope that the International Union for Co-operation in Solar Research will take the initiative in arrangement of regular observations of the total solar intensity with instruments which are compared with each other, and that at least at some stations, observations with absorbing substances may be arranged. Only the compensating pyrheliometer is convenient for the last purpose; but, as that instrument is already in regular use in several observatories—for example, in Vienna, Modena, Warschau and Upsala—I think that the adoption of this instrument will not meet with any difficulty. The spectrobolometric investigations of a limited number of water-cuvettes and glass-plates I will undertake with pleasure.

The observations respecting solar radiation which, from the beginning of 1905, have been going on at the physical institution of the University of Upsala are:—

1. Work with the compensating pyrheliometer, with and without absorbing substances.

2. Spectrobolometric registering of the infra-red part of the solar energy-spectrum, especially with respect to the influence of ozone.

3. Work with a quartz spectrograph for investigation of the changes in the limit of the ultra-violet solar spectrum.

4. Works with a new compensating instrument for determinations of the radiation of the earth, an investigation that will evidently be of interest for the knowledge of the atmospheric conditions, the changes of the earth-radiation principally depending on the presence of water-vapour and ozone.

No. 24.

Remarques sur l'état actuel des Recherches Solaires et sur les moyens de les améliorer.[1]

Par M. H. Deslandres.

Dans une Note des *Comptes rendus* de 1893 (t. CXVII., p. 716), j'ai réclamé l'enregistrement absolument continu des éléments variables du Soleil. La continuité a paru nécessaire pour reconnaître exactement l'influence exercée par les variations solaires sur la Terre, influence qui est certaine, mais encore mal déterminée. La matière solaire, qui se présente à nous formée de gaz et de particules, est en perpétuelle agitation, le mouvement général de rotation étant mis à part; et le rayonnement de chaque partie est constamment variable, comme aussi peut-être le rayonnement total de l'astre.

L'image du Soleil et de son atmosphère est fournie actuellement par trois groupes de méthodes et d'appareils qui donnent chacun une partie de l'ensemble.

La surface, qui est un nuage de particules, est relevée journellement avec l'œil ou la plaque photographique. Les observations sont nombreuses, mais non encore continues; le plus souvent aussi, elles sont isolées et ne sont pas reliées entre elles de manière à donner l'effet utile maximum.

[1] Cette note qui à été présenteé au Congres d'Oxford à été présenteé en même temps à l'Académie des Sciences de Paris le 11 Septembre, 1905.

Les mêmes remarques s'appliquent plus justement encore à l'atmosphère gazeuse du Soleil (appelée *chromosphère* ou divisée en couche renversante et chromosphère proprement dite), qui offre des mouvements et des variations d'éclat encore plus notables. La partie de la chromosphère qui est extérieure au bord est étudiée journellement avec l'œil depuis 1868 par la méthode de Janssen et Lockyer, dont le meilleur mode d'emploi est actuellement bien fixé.

La partie de la chromosphère, beaucoup plus étendue et importante, qui est intérieure au bord et projetée sur le disque, est décelée seulement depuis 1892 par des méthodes photographiques nouvelles, qui sont très générales, en ce sens qu'elles s'appliquent à toutes les vapeurs solaires, qui donnent une raie brillante ou noire, et aux couches différentes d'une même vapeur.[1] Mais elles exigent des appareils compliqués et coûteux (appelés communément *spectrohéliographes*) qui, même, n'ont pas encore trouvé leur forme définitive; car on discute encore sur le meilleur mode d'enregistrement des vapeurs et sur la meilleure disposition des appareils.

D'autre part, l'atmosphère solaire supérieure, constituée surtout par des particules et eppelée *couronne*, n'a pu encore être reconnue en dehors des éclipses totales et de la partie extérieure au bord. La lacune est regrettable, car c'est à la couronne et à ses jets de particules supposées électrisées et lancées par le Soleil avec une grande vitesse (ions de condensation et électrons), que l'on attribue l'influence des perturbations solaires sur la Terre. Ces jets doivent, il est vrai, être liés aux variations des couches

[1] Les méthodes en question ond été appliquées jusqu'ici seulement aux vapeurs de la chromosphère, or cette dernière couche contient aussi vraisemblablement des particules, qui n'ont pas encore été l'objet de recherches spéciales.

sous-jacentes de la surface, de la chromosphère et surtout de la chromosphère supérieure, et il suffit probablement d'enregistrer avec soin ces dernières couches.[1]

Pour toutes ces raisons, j'ai proposé en 1893 l'établissement, sur des points favorables du globe, d'appareils automatiques, à marche absolument continue, susceptibles d'enregistrer la surface du Soleil et les couches successives de son atmosphère. Ces appareils sont réalisables, mais avec une grande dépense, et le projet a été ajourné.

Par contre, il a paru facile d'organiser l'accord des observateurs actuels du Soleil, pour obtenir des images solaires bien comparables entre elles et à des moments différents. Dans ce but, la Société astronomique de France a nommé en 1900 une Commission solaire, chargée de publier des instructions et de centraliser les observations. Comme président de cette Commission, j'ai fait adopter par la Société les propositions suivantes, qui étaient présentées aussi aux sociétés étrangères d'Astronomie et que je soumets cette année au Congrès international de recherches solaires, convoqué prochainement à Oxford [voir l'*Introduction aux Instructions*, et l'*Observation journalière de la chromosphère et des protubérances* (*Bulletin de la Société astronomique*, septembre 1902 et avril 1905)] : 1° toutes ces images (dessins ou photographies) du Soleil et de son atmosphère ou tout au moins, les reproductions de ces images, auront, autant que possible, des diamètres qui soient égaux ou dans un rapport simple, qui soient par exemple des multiples, de 0 m, 10 ; ces images seront rapportées aux pôles du Soleil et rangées par rotations entières ; chaque image portant le numéro de la rotation et la longitude du méridien cen-

[1] Les nouvelles méthodes photographiques donneront peut-être un jour avec les raies très noires du spectre solaire, non chromosphériques, une image générale de la couronne entière.

tral[1]; 2° à une même station, l'observation aura lieu, autant que possible, à la même heure, et de manière que toutes les observations réunies soient réparties à peu près également dans les 24 heures; 3° les circonférences, développées en ligne droite, sur lesquelles on dessine les protubérances, auront aussi le diamètre des images précédentes.

La question solaire, comme je l'ecrivais en 1902, est, par sa nature même, d'ordre international, et il faut féliciter le grand astronome américain, Hale, d'avoir provoqué la création d'une Commission solaire internationale permanente. D'après le programme adopté, le Congrès de cette année doit, sur le terrain purement astronomique, se consacrer presque exclusivement au spectrohéliographe. Mais, à mon avis, on ne peut séparer les observations photographiques de la chromosphère des observations oculaires et des recherches sur les autres parties du Soleil. Il convient, surtout dans une première réunion, d'examiner la question dans son ensemble et de poser d'abord les règles générales applicables à tous les cas.

Remarques sur le spectrohéliographe.—Après ces considerations générales, je passe au cas particulier du spectrohéliographe qui offre un sujet d'études éminemment intéressant.

Il est question d'organiser la coopération de tous les appareils actuels, assez nombreux déjà, à une étude

[1] Dans les reproductions, on représente en général le Soleil comme on le voit à l'œil nu, lors du passage au méridien, c'est à dire le Nord en haut, l'Est à gauche; cette orientation naturelle pourrait être adoptée d'une manière definitive. La longitude du méridien central est évidemment une indication utile, elle est donnée dans les relevés ordinaires de la surface et des taches, et il convient de l'ajouter aussi dans les relevés de la chromosphère. Il est seulement désirable que les astronomes solaires s'entendent pour adopter une même durée moyenne de rotation pour les taches et la surface solaires.

méthodique, aussi continue que possible, avec les raies du calcium H et K et deux de leurs composantes H_1 et K_1, H_2 et K_2. L'idée est à tous égards excellente, mais sa réalisation, au moins avec les composantes brillantes H_2 et K_2, peut rencontrer quelques difficultés, sur lesquelles je dois insister, d'autant plus qu'elles ne sont pas indiquées dans les derniers Mémoires de Hale. Les appareils actuels ne donnent pas des images tout à fait comparables; car ils ont des dispersions différentes. Or la dispersion, qui élargit la raie astreinte à être enfermée dans une fente fixe, a une grande influence.[1] C'est ainsi que j'ai écrit en 1904 (*Comptes rendus*, t. CXXXIX, p. 337) :

"Les trois raies K_1, K_2, K_3 correspondent aux trois couches superposées qui sont la couche renversante, la chromosphère et la chromosphère supérieure. Or, avec une dispersion telle que les raies H et K sont écartées seulement de 0 mm., 4, le spectrographe a les trois raies réunies dans la seconde fente, et donc donne l'image des trois couches réunies. Avec un écartement de 2 mm., ainsi que dans l'appareil actuel de Meudon, on a seulement K_2 et K_3, c'est-à-dire la chromosphère proprement dite; puis, avec un écartement de 12 mm., K_3 est seule isolée, et l'on a la troisième couche seule. Avec la dispersion croissante, on peut éliminer progressivement la lumière des couches basses, et ne conserver dans l'image que la lumière des couches supérieures."

Les remarques précédentes sont vraies seulement dans leur ensemble, car elles ne s'appliquent qu'à la largeur moyenne des raies K_2 et K_3 supposée constante sur tous les points du Soleil et égale à la largeur de la fente du

[1] D'autres éléments interviennent aussi, à savoir : les rapports d'ouverture du collimateur et de la lunette, les largeurs des deux fentes, et aussi l'épaisseur et la forme de leurs joues, mais le cadre de cette Note ne permet pas de les examiner en détail.

spectrohéliographe. Or, ces raies ont, en réalité, une largeur très variable, surtout près des taches et facules, et il en résulte une complication nouvelle qui même est un des plus graves défauts du spectrohéliographe, signalé déjà depuis longtemps: l'image obtenue ou bien offre des intensités inexactes, ou est gâtée quelque peu par la lumière due à une couche différente.

Prenons les spectrohéliographes de Hale, Deslandres et Lockyer. Hale, après avoir employé de 1892 à 1894 la forte dispersion qui correspond à un écartement des raies H et K de 9 mm., est descendu à 4 mm. en 1903; de mon côté, j'ai adopté depuis 1893 la distance de 2 mm. (exactement $2^{mm,}$ 2 dans le dernier modèle de Meudon) et Lockyer a choisi 3 mm.

A priori, ces trois appareils peuvent donner des images différentes, surtout si l'on fait varier la largeur de la seconde fente. Hale, dans le choix de la dispersion en 1903, s'appuie sur un calcul de Michelson (*Astrophysical Journal*, 1895, p. 1), qui recherche le contraste maximum entre la raie brillante K_2 et le fond brillant continu sur lequel elle se détache. Mais ce calcul s'applique à l'observation des vapeurs sans la seconde fente et non au spectrohéliographe qui, avec sa seconde fente, élimine le fond continu et qui, d'ailleurs, donne une belle image même lorsque le contraste est nul. De plus, Michelson admet pour la raie K_2 une largeur de 0Å, 25; or, au fait, la larguer moyenne est de 0Å, 40 à 0Å, 50 et varie de 0Å, 25 à 1Å. Pour ma part, j'ai choisi la dispersion de 2 mm. qui permet de renfermer la raie K_2 entière dans une fente de $0^{mm,}06$ à $0^{mm,}07$.

Cependant la raie K_3 est aussi parfois large, presque aussi large que la raie K_2 et, dans ce cas, surtout avec la seconde fente fine, on a des plages non plus brillantes, mais noires par rapport au fond. On obtient ainsi ce que

Hale a appelé *dark calcium flocculi*, même avec la petite dispersion de 2 mm.

Si donc, avec les spectro-héliographes, on veut avoir des résultats comparables, il faut réglementer avec soin le choix des dispersions et des largeurs des fentes;[1] et, pour la même raison, j'ai réclamé déjà l'indication sur chaque épreuve, de la largeur des deux fentes en millimètres et de la largeur de la seconde fente en "Ångströms." Mais quelle est la dispersion la meilleure? Elle varie avec la largeur adoptée pour la raie K_2, largeur qui est variable elle-même d'un point à l'autre du Soleil et qui croît régulièrement vers le bord. L'image obtenue est parfois difficile à interpréter, d'autant que les variations des raies en largeur sont attribuables, soit à la densité de la vapeur, soit à son épaisseur, soit à l'excitation électrique. Les trois couches K_1, K_2, K_3 sont vraisemblablement dans des états physiques très différents, et on peut critiquer le prélèvement partiel de la lumière d'une couche et le mélange avec une autre couche, qui se produisent dans des proportions variables surtout avec une dispersion un peu forte. Aussi je donnerais volontiers la préférence à une dispersion plus faible que les précédentes, qui écarte H et K par exemple de $0^{mm}.7$, qui réunit sur la même épreuve toute la lumière de K_2 et K_3 et la meilleure partie de K^1 et qui donne l'image de l'atmosphère gazeuse dispersion et l'augmentation de lumière qui en résulte

[1] Le spectrohéliographe, comme certains appareils déjà anciens de photographie panoramique, forme son image par le mouvement d'une ligne lumineuse limitée par une fente. Or dans l'appareil panoramique, la fente peut, sans inconvénient être un peu large, surtout lorsque l'appareil tourne exactement autour du point nodal postérieur de son objectif. Mais, avec le spectrohéliographe, il faut diminuer autant que possible les larguers des deux fentes, parce que les spectres des différents points de la première fente se superposent dans la seconde fente.

entière séparée de la photosphère.[1] Cette image échappe aux critiques précédentes et est très lumineuse. La faible permettraient d'avoir des images aussi grandes que les belles images actuelles de la photosphère (0^m 20 et plus) et avec les mêmes objectifs astronomiques.[2] Elle convient aussi pour les objectifs à long foyer qui sont employés avec un sidérostat, et ont de grands avantages.

Les spectrographes précédents, que j'ai appelés *spectrographes des formes*, sont les seuls employés par Hale. Or, depuis 1891, j'emploie, en même temps, d'autres spectrographes, dits *des vitesses* ou *par sections*, qui sont à l'opposé des précédents et qui, pour cette raison, fournissent justement ce que les autres ne peuvent pas donner et donc les complètent heureusement. Ils ont en effet une dispersion très forte, un mouvement discontinu et une seconde fente large qui comprend les trois composantes de K. Ils donnent les largeurs exactes des raies K_2 et K_3, les épaisseurs des couches correspondantes et aussi leurs vitesses radiales et leurs mouvements relatifs. Les premiers fournissent l'image d'ensemble da la chromosphère, les seconds, tous les détails des couches composantes et en particulier de la couche supérieure, la plus intéressante, qui, à cause des variations plus grandes de K_3, en accord avec la hauteur plus grande dans l'atmo-

[1] Dans l'autre partie de K_1, le spectre continu de la photosphère qui se mêle à la lumière de la vapeur prend une importance trop grande.

[2] L'appareil, fixé à un équatorial, pourrait être ainsi constitué : objectif astronomique ; lentille simple en crown au zinc, dont l'indice ne varie pas avec la température, ouverture de 0 m., 20, distance focale 3 m. ; lentilles du collimateur et de la chambre simples aussi et en crown, distance focale 1 m, 20 ; prisme en crown a petit angle, argenté sur une face de manière à renvoyer le spectre vers l'objectif astronomique ; objectif qui agrandit six à sept fois la seconde fente, supposée parfaite ; pose de 2 minutes pendant le passage du Soleil sur la première fente, par le mouvement diurne, la plaque photographique seule étant mobile. Les variations dues au pouvoir absorbant de notre atmosphère seraient compensées par l'emploi de plaques ayant des sensibilités differentes.

sphère, échappe au spectrohéliographe des formes. Aussi, en 1905 comme en 1891 et 1893, j'estime les deux spectrographes nécessaires à une étude complète des vapeurs chromosphériques, et, autant que possible, chaque jour, je fais une épreuve avec chacun des appareils. Il est seulement désirable que les images solaires, larges au plus de 0^m 10, soient agrandies et au moins doublées.

D'ailleurs, le spectrographe par sections, avec une grande image du Soleil, large de 0^m 20 et avec 400 sections donne assez bien la forme générale des vapeurs et peut suppléer le spectrographe des formes; il se prête mieux aussi à un enregistrement absolument continu.

A Meudon, les deux spectrographes de petite et de grande dispersion sont montés sur la même table qui peut à volonté avoir un mouvement continu ou discontinu, et chacun peut être employé indistinctement à l'enregistrement des formes ou des vitesses radiales. Les deux spectrographes ont donné des photographies des formes avec des raies noires du spectre solaire, autres que H et K. Cette étude, que j'ai inaugurée en 1894, constitue un champ nouveau extrêment vaste à explorer; elle nous décélera la répartition exacte des diverses vapeurs dans le Soleil.[1]

Le spectrographe des vitesses se transforme aisément en un autre spectrographe appelé *enregistreur de spectres* et employé autour des taches, qui opére aussi par sections, et juxtapose non plus une petite portion du spectre près de la raie K, mais de larges étendues du spectre photo-

[1] Elle m'a conduit à penser que les grains de la photosphère et les intervalles relativement noirs entre les grains ont des différences dans leurs spectres et leur composition chimique. Mais cette distinction curieuse, pour être reconnue nettement, exigera des appareils plus puissants que les miens employés dans des conditions atmosphériques très favorables.

graphique.[1] Autour des taches, les changements du spectre, très forts avec les raies du calcium, s'étendent plus ou moins à toutes les raies solaires et au spectre continu et sont absolument dignes de fixer l'attention. En fait, dans l'organisation adoptée, les enregistreurs des formes, des vitesses et des spectres se prêtent un mutuel appui.[2]

No. 25.

Sur le sens précis de mots anciens et sur le choix de mots nouveaux.

Note de M. H. Deslandres.

Une des tâches secondaires qui incombent au Congrès paraît être la détermination du sens précis de certains mots, et le choix de mots nouveaux.

Je soumets au Congrès les remarques suivantes:

Le mot *chromosphère* a été choisi pour désigner la couche assez épaisse de gaz légers qui est visible tous les jours et sur tout le bord solaire avec le spectroscope. Puis on a découvert la *couche renversante*, très mince, interposée entre la chromosphère et la surface, formée de gaz plus nombreux et plus denses.

On écrit souvent: la couche renversante forme la partie basse ou la base de la chromosphère.

[1] En général, autour des taches et sur les taches, on étudie le spectre de longueur d'onde supérieure à 5000; mais le spectre de longueur d'onde inférieure offre aussi des détails curieux. On peut avec l'appareil enregistrer les deux régions.

[2] Ainsi, les enregistreurs des spectres montrent que l'augmentation d'éclat audessus des facules, observée avec les enregistreurs des formes et les raies telles que 404,5 du fer, proviennent non de la raie centrale très noire, mais des parties dégradées de chaque côté ou peut-être même du spectre continu immédiatement voisin. L'image avec la raie centrale seule aurait un grand intérêt; car elle est liée peut-être à la couronne projetée sur le dique.

Peut-on admettre cette extension donnée ou mot chromosphère? Ou convient-il d'adopter un mot nouveau pour désigner l'ensemble des deux couches?

Dans ce dernier cas, je propose le mot *gazosphère*, qui a l'avantage de rappeler la propriété caractéristique de la partie basse de l'atmosphère solaire constituée principalement par des gaz; alors que la partie haute, appelée *couronne*, est constituée surtout par des particules.

J'appelle aussi l'attention sur le mot *protubérance*, qui, suivant les auteurs, désigne seulement la masse gazeuse audessus de la chromosphère, ou, à la fois, cette masse gazeuse et sa base dans la chromosphère. Il semble difficile de séparer ces deux dernières parties, surtout si l'on attribue le phénomène à une éruption.[1] En tout cas, pour éviter toute ambiguïté, il sera bon de définir le sens précis du mot protubérance.

Récemment, il a fallu créer des mots nouveaux pour désigner les appareils qui donnent des images monochromatiques et les détails de ces images.

Dès le début, Hale a adopté pour les appareils le mot *spectrohéliographe*, qui peut être rapproché du mot ancien assez peu usité de *photohéliographe*, les autres spectrographes destinés au Soleil pouvant alors être appelés *héliospectrographes*. Cependant l'appareil s'applique à une source lumineuse quelconque. Aussi ai-je adopté d'abord un autre mot: *spectrographe enregistreur à deux fentes*, qui indique un caractère important de l'appareil, mais est un peu long. L'année dernière j'ai proposé l'examen d'autres mots: *spectromobilographe* ou *spectromobile*, qui rappellent un autre caractère de ces appareils.

Mais, après réflexion, je m'arrêterais plutôt au mot

[1] Les astronomes qui relèvent journellement les protubérances par l'observation oculaire, désignent en général par le mot protubérance la masse gazeute entière, du sommet à la surface du Soleil.

spectroenregistreur ou *spectroregistreur* qui indique que les appareils sont automatiques, et s'applique également bien aux divers types qui enregistrent séparément soit les formes des vapeurs soit les vitesses radiales, soit les spectres entiers.[1] On peut objecter, il est vrai, que le mot spectrohéliographe est à peu près consacré par l'usage.

En tout cas, au mot adopté, on ajourterait les indications brèves suivantes: à deux ou trois fentes, monochrome ou polychrome, des formes ou des vitesses, lesquelles donnent une idée précise de la nature et du but de l'appareil.

Quant aux images de ces appareils, les premières, dues aux raies brillantes du calcium, ont été appelées par Hale *photographies de facules*, les grandes plages brillantes des vapeurs étant en accord avec les facules bien visibles de la surface; les petites plages brillantes formaient le *réseau faculaire*. De mon côté j'ai employé les mots: *photographies de la chromosphère* et *réseau chromosphérique;* j'ai appelé les plages brillantes *flammes faculaires* et, l'année dernière, j'ai proposé le mot *faculide*.

Depuis 1903, Hale, abandonnant les mots anciens, désigne simplement par le mot *flocculi* les plages brillantes des vapeurs, petites ou grandes. Le mot *flocculi* a l'avantage de rappeler l'aspect des petites plages; mais il ne rappelle pas le lien étroit, actuellement bien établi, avec les facules de la surface.

A mon avis, il convient de conserver les trois mots *flocculi, faculide, réseau*,[2] pour désigner les petites plages,

[1]Le mot spectrohéliographe peut être conservé pour désigner, comme à l'origine, l'appareil special qui donne les formes des vapeurs, et le mot spectroenregistreur, plus général, serait adopté pour désigner tous les appareils enregistreurs des formes, des vitesses et des spectres.

[2]Ces grandes plages sont tellement différentes des petites qu'un mot spécial me paraît necessaire pour les désigner. Le mot réseau doit aussi être conservé, car l'aspect de l'image, avec les poses employées d'ordinaire, est bien celui d'un réseau. Dans les cas où la raie isolée est la raie brillante K_2, le réseau doit être appelé chromosphèrique, car il correspond certainement à la chromosphère proprement dite.

les grandes plages et l'ensemble de toutes les petites plages.

Les images seraient appelées *images de telle vapeur et de telle raie*, en ajoutant, lorsque la couche représentée est bien déterminée, images de la couche renversante, de la chromosphère, de la chromosphère supérieure, ou encore de la gazosphère.

Je prie ceux de nos collègues qui ont des idées particulières sur les points soulevés, de vouloir bien les communiquer.

No. 26.

Proposal for an investigation of the local inequalities of the Sun's radiation field.

By W. H. Julius.

I beg leave to make a few suggestions relating to Mr. Abbot's very interesting statement, under (*e*) above (p. 99), entitled: Variability of the Sun.

Spectro-bolometric determinations of the solar radiation, combined with other kinds of observations, have led Prof. Langley to make the very remarkable inference that the so-called "solar constant" appears to be subject to variations of 10 per cent. and more.

I quite agree with Prof. Abbot that it is important to verify this conclusion by independent observations; but even if it should be found that the solar constant really varies, we need not conclude, I think, that these variations are necessarily caused by a corresponding variability of the solar output. The first thing, indeed, we want to know is, whether the solar radiation varies merely *with time*, or also, for a given distance, *with place*.

If we start with the supposition that the solar constant *must* have the same value everywhere at a given distance

from the Sun and at a given instant, but changes only with time, then, of course, we are forced to ascribe the observed phenomenon to a corresponding variability of the emitting or absorbing powers of the Sun's outer layers. Another possible supposition, however, is *that the solar radiation is unequally distributed in different directions.* If such were the case, apparent variations of the solar constant would be observed on the Earth, even when the conditions on the Sun remained entirely unaltered.

Now there is strong evidence that inequalities in the Sun's field of radiation do really exist.[1] They result from ray-curving in the strata and vortices of the solar gases, and must occur *at any distance* from the Sun. The order of magnitude of these inequalities depends on the refractive indices of the solar gases, and, therefore, must be especially very different for those various kinds of light lying near absorption lines suffering anomalous dispersion.

Whether the local inequalities of the solar constant, to which *all* waves contribute, will prove large enough to be detected, we can neither assert nor deny *a priori.* If *rapid* changes of the solar constant should be observed, it would be much simpler to explain them by the above-mentioned local inequalities of the radiation field than by changes on the Sun. And it might even be possible to detect local inequalities of the Sun's radiation field *directly* by comparing simultaneous observations made in widely separated stations on Earth.

Co-operation is necessary to settle the question. A number of identical equipments would be required, each consisting, *e.g.*, of a very promptly indicating thermopile with dead-beat galvanometer, giving a continuous photo-

[1] Cf. *Physik. Zeitschrift* 6, 239—248; *Revue gén. d. Sciences,* xv., p. 480—495; *Proc. Roy. Acad. Amsterdam* 6, p. 270—302, or *Arch. néerl.* [2], ix., p. 211—250.

graphic record, or of a Callendar absolute recording bolometer. These instruments are to be placed in two or more groups, if possible near the same meridian. The distance between the groups should be very great; the distances between the instruments of each group should only be great enough to have them placed under different sky conditions, so that, by comparison of their curves, cosmical disturbances may be distinguished from terrestrial ones. Only those variations common to all actinometric records of the same group are to be compared with the variations common to the records of another group.

If the above suggestions meet with approval I would propose that a committee be appointed in charge of discussing and elaborating a plan for arranging the observations.

No. 27.

Sur le mouvement de la matière à la Surface Solaire.

Par Prof. A. Bélopolsky.

L'élargissement des raies, qu'on observe visuellement dans le spectre des taches solaires se reproduit difficilement au moyen de la photographie. Quand le spectre de la tache et de la photosphère sont près sur la même plaque avec la même pose, il semble que les raies spectrales sont différentes dans les deux cas. Cependant si la pose du spectre de la tache augmente, alors la différence devient insensible. J'ai l'honeur de montrer ici quelques photographies des spectres de grandes taches de cet été que j'ai prises au moyen de notre spectrographe stellaire avec un obturateur de Hartman. La partie du milieu présente le spectre du noyau; des deux cotés se trouve le spectre de la photosphère. Le 30 pouce fut fixé sur la tache au moyen

d'un chercheur avec un grossissement de 400. On ne voit pas des différences sensibles.

Les observations anciennes et les théories nouvelles du soleil indiquent des grandes vitesses de la matière, abandonnant le soleil. Ce serait bien interessant de trouver la valeur numérique de cette vitesse en se basant sur le principe Doppler. Les travaux de M. Deslandres sur ce sujet sont bien connus. Moi j'ai taché aussi de faire quelques recherches dans le même but.

Les grandes vitesses doivent avoir la direction du rayon solaire. C'est donc au milieu du disque, qu'on doit chercher principalement l'influence sur le déplacement des raies spectrales. Mais sur aucune de mes épreuves du spectre dans les voisinage même des grandes taches j'ai pu trouver, un déplacement sensible. Ce sont les raies bien connues H. et K. que j'ai soumis aux recherches. Cependant quelques epreuves prises sur le bord solaire avec une pose assez longue m'ont donné des déplacements des raies correspondantes aux vitesses des 30 a 90 km/sec. Presque chaque grande tache, ou facule vive, quand elles se sont approchées du bord donnaient dans leur voisinage des spectres dans lesquels les raies H. et K. étaient deformées. Le temps de pose pour ces détails etait de 30s.—60s. Avec une telle longue pose le spectre du disque solaire devient tout a fait surexposé. Ainsi je crois que la cause que près du centre on ne peut pas avoir les déplacements des raies H. et K., est dans ce que les parties deplacées sont très faibles.

On peut se demander, quelles sont les vitesses vraies, correspondantes aux vitesses radiales détérminées au bord solaire. Ici on peut faire deux hypothèses: ou que la matière se dissippe dans toutes les directions dont l'une coincide au rayon, ou que se sont les particules, qui se meuvent radialement, mais que ce rayon fait quelque petit

angle avec le rayon normal à la ligne de vision. En posant, que la hauteur de la matière H. et K. et de 30" on peut conclure, que l'angle maximum entre les deux rayons solaires est à peu près 15°, ainsi la vitesse vraie pourrait être six fois plus grande, que la vitesse trouvé (c.à.d. 300 km.). On peut aussi détérminer la vitesse près du bord, si on fait une serie de photographies d'une protubérance. En mesurant la longueur de H. et K. on pourrait trouver les changements correspondants aux vitesses. Pour ce but il faut un refracteur, de grandes dimensions.

PART VII.

Paper accepted by the Executive Committee for Publication in the Transactions.

No. 28.

Observations of the Spectra of Sun-spots, Region b to E.

By
A. Fowler.

Method of Observation.

A previous discussion of the spectra of sun-spots in the region C to D,[1] based on observations made in 1903–4, led me to the conclusion that the most fruitful plan in future work would be to restrict the observations to a comparatively small part of the spectrum, and to endeavour to record every detail which my instrument was capable of showing. During 1905 and the first three months of 1906, I adopted this method of work, and obtained the results which form the principal subject of the present communication. The important paper on the photographic spectra of spots recently published by Prof. Hale and Mr. Adams[2] gives additional interest to the observations, as permitting a comparison between photographic and visual records.

The instrument employed was that previously used for the region C to D, namely, an Evershed two-prism solar spectroscope attached to a 6-inch equatorial. The instrument is particularly well adapted for the observation of prominences and their spectra, but it has sufficient resolving power to exhibit most of the details in the spectra of spots. With careful adjustment, the purity of

[1] *Monthly Notices*, vol. lxv., pp. 205, 513.

[2] *Astrophysical Journal*, vol. xxiii., p. 11, January 1906.

spectrum attainable is about 25,000 in the neighbourhood of b. On account of the greater dispersion in the green, the part of the spectrum selected for minute examination was that extending from b_4 to E_1, in which Rowland tabulates about 350 solar lines.

The wave-lengths were read off from the maps of Rowland or Higgs, and corrected by the tables in the usual manner, the positions of spot lines not clearly seen in the maps being arrived at by eye-estimates. In the case of identifications with very faint solar lines, the wave-lengths tabulated are not to be supposed to mean more than that the lines could not be distinguished in position from the wave-lengths given. They may or may not be actually identical with the solar lines. The intensities of the various lines in the spot spectrum, meaning thereby the combined effects of widening and darkening, were noted by comparison with Fraunhofer lines outside the spot spectrum, in accordance with a suggestion put forward in my former paper. This method has also been adopted by Hale and Adams, and the comparison of the photographic and visual observations is thereby greatly simplified.

At the beginning of this new series of observations, independent lists of the affected lines were made from day to day, but the essential constancy of the spectrum, even in minute details, soon forced itself upon me, and the method of observation was changed. All the data were collected into one catalogue, and a map of the spectrum was made, being gradually completed by the insertion of additional lines as their positions were determined. The spot spectrum was drawn in the first instance on a strip of tracing cloth pinned across the map, but after checking and correction it was drawn directly on an enlarged bromide copy of Higgs's photograph of this part

of the spectrum (see Fig. 2). Subsequent observations were thus reduced to looking for differences between the actual spot spectrum and the map and table.

During the progress of the observations a valuable account of the sun-spot spectrum as seen with a very powerful equipment, was published by W. M. Mitchell, of Princeton.[1] Certain details in connection with close doubles which might not have been discovered independently were clearly recognised when attention was drawn to them. The thinning, or almost complete obliteration, of the component 5169·22 of b_3, giving the appearance of a notch on the less refrangible side of the line, is a case in point. Somewhat similar appearances were subsequently detected in other lines.

Types of Spot Lines.

The various types of lines presented to the observer have been well described and illustrated by Mitchell (see Fig. 1). They include the varieties:—

(1) Widened and darkened lines.

(2) Widened and weakened lines.

(3) Widened lines, with centres reversed bright.

(4) Winged lines.

(5) Thinned lines, sometimes amounting to obliteration.

(6) Hazy short lines, or "band lines," which are usually restricted to the umbræ of spots. To avoid misconception, the name "umbra line," suggested by Mr. Newall, is adopted for these in the present paper.

At rare intervals, as on February 2, 1905,[2] there are bright reversals of a great many lines besides those to

[1] *Astrophysical Journal*, vol. xxii., p. 4, July 1905.

[2] *Monthly Notices*, vol. lxv., p. 513.

which reference is made by Mitchell as being usually, if not always, present as such.

Besides these phenomena, there are the numerous bright streaks which interrupt the continuity of the general darkness of the spot spectrum, many of which can be traced as bright interspaces between the fainter lines of the ordinary solar spectrum away from the spot. Those at 5163·7 and 5168·6 are notable examples.

As regards the " widened and weakened " lines, Mitchell describes them as similar in appearance to widened lines except that the widened part is not so dark as the normal line, and states that with low dispersion such lines often seem to disappear in the spot spectrum. The latter is usually my experience, but 5196·2, stated by Mitchell to be usually a weakened line, appears to me as an ordinary widened line. The reversed lines, apart from the exceptional cases which probably arise from overlying prominences of great brilliancy, appear to be beyond the power of my instrument. Reversals of this kind have sometimes been suspected, particularly in the case of the two Iron lines 5191·63, 5192·52, and the three strong Chromium lines 5204·68, 5206·22 and 5208·60. The first of these is marked " Revd?" by Mitchell; the second is not mentioned by him, and the last three are marked " thinned and winged." It is evident therefore that some account must be taken of the instruments employed when comparing the results of different observers. From this point of view, the photographic results of Hale and Adams, in which the highest purity of spectrum is not realised on the photographic plates, afford a better comparison with my observations than those of Mitchell, and this is especially so because the latter observer has worked over a great range of spectrum, and has probably not recorded every detail.

The appearance presented by a line in a spot would seem to depend to a great extent upon the character of the line as seen in ordinary laboratory experiments. Lines of calcium, for example, are nebulous in arc spectra, and they appear widened and winged when intensified in spots. The lines of titanium, on the other hand, are mostly well-defined in the arc, and the corresponding lines in the spots are more darkened than widened.

The " umbra lines " appear to be always visible, but it is often difficult to determine whether an umbra line or a widened line of the ordinary type is under observation, and mistakes on this point may have been made. The positions of these lines, and of the bright streaks which separate them, were determined with the view of attempting to trace the substance which produces them. On some occasions, when the umbra of a spot was not very dark, there have been indications of a greater number of faint umbra lines than those tabulated, representing no doubt, what is ordinarily understood by the resolution of the general darkening due to the spot; in view of the results of Hale and Adams, it is possible that these may be slight intensifications of the very faint lines not ordinarily traced in the solar spectrum with my instrument.

It should be remarked that the catalogue includes all the lines which appeared to be widened or darkened, irrespective of whether the altered appearance may be real or subjective. It is believed, however, that the effects noted are mostly real, as there is no obvious reason why some lines should be affected, while others of equal intensity in the same field remain unchanged, if the observed appearances were to any great extent subjective. The subjective widening of lines probably diminishes with increased experience on the part of the observer.

Assignment of Origins.

For the assignment of origins to the various lines, Rowland's identifications of the corresponding solar lines have been generally followed. Various corrections and additions have, however, been made by reference to subsequent work on the arc spectra of the elements; chiefly to the tables of lines in vanadium, titanium, chromium, and cobalt published by Hasselberg, and collected in Watts' "Index of Spectra." In addition, a series of photographs has been taken for the purpose of checking discrepancies between Rowland and Hasselberg, and for the further determination of enhanced lines. Any departure from Rowland is indicated by H, K R, L or F following the symbol of the element, respectively indicating Hasselberg, Kayser and Runge, Lockyer, and Fowler.

It will be seen that much further research is necessary to complete the identification of the lines.

Description of Catalogue.

Excluding the probably superficial phenomena of reversals, the spectra of all the numerous spots examined were essentially the same in the region investigated, and the map and catalogue have now been checked so often that I can confidently regard them as representing the spot spectrum during 1905, as seen with the dispersion of my instrument. The map, however, is necessarily to some extent diagrammatical, and should be regarded rather as an aid to observation than a true representation of the spectrum. The excellent agreement between the map and the photographs at Mount Wilson has increased my confidence in the observations, and I believe that the map and catalogue together may safely be used for enquiring

into possible changes as the next spot minimum approaches. It is quite possible that the intensities of the spot lines, referred to the solar lines, showed some variations from spot to spot, but if so, they appear to have varied as a whole and not individually.

The first, second, and third columns of the catalogue give respectively the wave-lengths, origins, and intensities of the corresponding solar lines, taken directly from Rowland's tables unless otherwise indicated. Various notes as to the origins will also be found in the column of remarks. The fourth column gives the wave-length and solar intensity of a solar line to which an adjacent spot line approximates in intensity. Column five shows the intensities of the lines in spots (including umbra lines only as far as $\lambda 5212$) as tabulated by Mr. Adams from the Mount Wilson photographs; the intensities are on Rowland's solar scale. In the column of remarks "u" indicates "umbra line." References to Mitchell's observations are shown by the letter M followed by a number; these numbers do not indicate intensity on the solar scale, but the relative conspicuousness of change in the appearance of a line; negative numbers indicate weakening or thinning, -5 meaning complete obliteration.

DETAILS OF SUN-SPOT SPECTRUM, *b*—E.

Wave-length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5166·13		0000	64·72 (1)		*u*
66·45	Cr, Fe	3	62·45 (5)	3-4	M 4
67·0			64·72 (1)		*u* . . M 9 *u* (66·9)
67·50	Mg	15			b_4 Occasionally revd. bright
68·36		000	76·74 (1)	00 *u*	*u* . . M 5 *u* (68·3)
68·6					Bright streak, traced on sun
68·83	Ni	1			M – 3
69·07	Fe	3		]5, weak on red edge	b_3 Compound line appears notched on red side, M – 4 for 69·22
69·22	*p* Fe (L)	4			
69·47		00	70·94 (0)	·66 = 000 *u*	*u*
69·8					Bright streak
70·27		0000	70·94 (0)	000 *u*	*u*
70·5					Bright streak
70·77		000	76·74 (1)	0 *u*	*u*
71·19		0000	76·74 (1)	00-0 *u*	*u*
71·78	Fe	6	7-8		Darkened
72·39		00	76·74 (1)		*u*
72·86	Mg	20			b_2 Occasionally revd. bright
73·50		0000	76·74 (1)		*u*
73·92	Ti	2	91·63 (4)		
74·60		000	70·94 (0)		*u*
75·10		000	70·94 (0)	00 *u*	*u*
75·42		000	]76·74 (1)		]*u*
75·58		000		00-0 *u*	
75·92		000	77·41 (0)	00 *u*	*u*
76·19	Co	000	]77·41 (0)		
76·31		000		00-0 *u*	
76·5					Bright streak
76·74	Ni	1	70·94 (0)		Weakened
76·95	V	000	76·74 (1)	0	M 7
77·41	Fe	0	76·74 (1)	] 1-2	M 4, darkened. Co not confirmed by H. Cr confirmed by F
77·58	Co, Cr (H)	00	77·41 (0)	] hazy	
78·16		0000	77·41 (0)		*u*
78·64		000	76·74 (1)	00 *u*	*u*
78·97		00	80·23 (1)		M 4 *u*. R's intensity low
79·29		000	,, (1)		*u*
79·5					Bright streak
79·70		0000	78·97 (0)		*u*
79·8					Bright streak
80·75		000	80·23 (1)	·66 = 0 *u*	*u*
81·50		00	,, (1)		
82·01		000	]78·97 (0)		]*u*
82·12		0000			
82·6					Bright streak
82·91		000	78·97 (0)		*u*
83·79	Mg	30			b_1 Occasionally revd. bright
84·45	Fe	2	86·07 (2+)		M 5, hazy. R's intensity high
84·74	Fe, Cr, Ni	1	84·45 (2–)		

Wave-length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5185·20		0000	80·23 (1)	000-00 *u*	*u*
85·9			,, (1)		*u*
86·07	*p* Ti (F)	2	,, (1)		Thinned
86·50	Fe, Ti (H)	000	,, (1)	00 *u*	Combine to give appearance of a line displaced to violet, M – 3 for 86·07. Fe given by R only, Ti confirmed by F
86·72		000	< ,, (1)		*u*
87·0					Bright streak
87·62		000	< ,, (1)	00 *u*	*u*
88·08	Fe	1	Weakened	0-1	M – 3
88·23		0000	Widened		*u*, M 4 *u*
88·86	*p* Ti (L)	2	Weakened	6, hazy towards violet	Compound line appears notched on violet side. M 2 for ·86
89·02	Ca	3	Widened		
89·50		0000	81·50 (00)		*u*
89·74		000	,, (00)		*u*
90·3			88·08 (1)	00-0 *u*	*u*. H and A give 90·35
90·5					Bright streak, traced on sun
90·8			88·08 (1)	00-0 *u*	*u*. H and A give 90·73
91·1					Bright streak
91·63	Fe	4	92·52 (5)	5, hazy	Bright revl. sometimes suspected ; M 3, revd. ?
92·16	Cr	00	88·08 (1)	0	
92·52	Fe	5	6-7		Bright revl. sometimes suspected
93·14	Ti	2	95·11 (4)		May include V 93·18
93·67	Cr	000	98·11 (0)	·61 = 00 *u*	
93·9					Bright streak
94·22	Ti	000	96·23 (1)	·14 = 0-1 *u*	
94·5					Bright streak
94·7			98·11 (0)		*u*
95·11	Fe	4	> 92·52 (5)	5, winged	Winged ; probably includes V 95·02 ; M 5 winged
95·65	Fe	2	< 98·89 (3)		M 4 winged
95·9					Bright streak
96·23	Fe	1	95·65 (2)	2, fringe on v. edge	M – 3, usually weakened
96·61	Cr	0	,, (2)	1, widg. prob. due to Mn	M 5, darkened. Kayser gives Y at 5196·59
96·74	Mn	00			
97·0					Bright streak, traced on sun
97·33	Ni, Mn	00	96·23 (1)	0-1	
97·74	*p* Fe (F)	2	98·11 (0)	0-1	Nearly oblitd.
98·11		0	96·23 (1)		M 5
98·51		0000	98·11 (0)	00 *u*	*u*. M 8 *u* at 98·7
98·89	Fe	3	> 95·11 (4)	4	M 6, hazy
99·77		000	96·23 (1)	0-1 *u*	
99·88		000			
5200·36	Cr	00	00·59 (0)	00-0	M 3
00·59	Y (K R)	0	Not affected		M 4, darkened. R gives V, but not confirmed

Wave length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5201·26	Ti	000	00·59 (0)	0	
01·6					Bright streak
01·77		0000	00·59 (0)	00-0 *u*	*u*
02·44	Fe ?	2	>06·22 (5)		
02·52	Fe	4			M 5, winged
02·95		0000	96·23 (1)		*u*
03·12		0000			
03·4					Bright streak
03·66		000	96·23 (1)	0 *u*	*u*
04·68	Cr	5	>69·72 (8)	10, fringe on v. edge	Probably frequently revd. bright. M 5, thinned and winged
04·77	Fe	3			
05·11		0000	00·59 (0)		*u*
05·6					Bright streak, traced on sun
05·9	Y	0	? oblitd.		M – 4
06·22	Cr, Ti	5	69·72 (8)	6	Probably frequently revd. bright. M 5, thinned and winged
06·71		000	98·11 (0)		*u*
07·1					Bright streak
07·26		0000	00·59 (0)		*u*
07·9					Bright streak
08·11	Ti	00	00·59 (0)		
08·60	Cr	5	69·72 (8)	6	Probably frequently revd. bright. M 5, thinned and winged
08·78	Fe	2			
09·2			00·59 (0)		*u*
09·6					Bright streak
09·95		0000	00·59 (0)		
10·06		000			
10·3					Bright streak
10·56	Ti	3	06·22 (5)	5, hazy	M 5, darkened
11·02		000	18·37 (1)	0 *u*	*u*
11·11		0000			
11·5					Bright streak, traced on sun
11·70	Fe	00			Weakened or unaffected. Fe given by R only
12·1					Bright streak
12·40	Cr (F)	000	18·37 (1)	0 *u*	
12·50	Ti (H)	0000			Ti confirmed by F
12·86	Co (H)	000	18·37 (1)	0	Hazy
13·52		000	18·09 (0)		
14·29	Cr	00	„ (0)	0	Hazy
14·5					Bright streak, traced on sun
14·78		00	18·37 (1)	0	Hazy
15·35	Fe	3	<95·11 (4)		
15·74		000	18·09 (0)		
16·44	Fe	3	06·25 (5)		Winged. ? often revd. bright
17·0			18·09 (0)		
17·55	Fe	3	<95·11 (4)		
17·9					Bright streak
18·09	Cu (K R)	0	18·37 (1)	0-1	M 5. Fe given by R
18·37	Cu (K R)	1	<25·70 (2)		M 5. Fe given by R, Co by H

Wave-length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5218·8					Bright streak, traced on sun
19·19		0000	19·88 (0)		*u*, double
19·88	Ti	0	25·70 (2)	Int. not stated	M 10
20·25	Cu (K R)	000	] 18·37 (1)		M 3
20·36	Ni	0			
21·08	Cr (H)	000	] 18·37 (1)		Cr confirmed by F
21·20	Cr	00			Cr not confirmed by H or F
21·93	Cr	0	18·37 (1)	1	M 4
22·56	Cr	00	<18·37 (1)	00-0	M 3. Cr not confirmed by H or F
22·85	Ti, Cr	00	18·37 (1)	0	M 3
23·1					Bright streak
23·35	Fe	0	] 18·37 (1)		M3, darkened
23·53	Ti	0000			M 5, darkened. Ti not confirmed by H or F
23·79	Ti (H)	000	28·55 (1)	0	M 3. Ti confirmed by F
24·24	Cr	000	24·47 (0)	00-0	
24·47	Ti	0	25·70 (2)	1	M 5, darkened
24·71	Ti, Cr	00	28·55 (1)		M 4
25·10	Cr	0	] 34·79 (2)	] 1-2	
25·20	Cr, Ti, Fe	00			M 7. Fe given by R only
25·70	Fe	2	<30·03 (4)	3	M 6, revd. twice
25·97	Cr	000	28·55 (1)		M 5, revd. beyond umbra once
26·36		0000	24·47 (0)		*u*
26·4					Bright streak
26·71	*p* Ti (L)	2	28·55 (1)		M oblitd. once ? revd. once ? (2 observations)
27·04	Fe, Cr	3	30·03 (4)	4	M – 4, weakened 3 times. Fe chief component
27·36	Fe	5	33·12 (7)		Much darkened
27·90		000	24·47 (0)		*u*
28·27	Cr	000	,, (0)		
28·55		1	25·70 (2)		M 3
29·4			28·55 (1)		*u*, probably double
30·38	Co, Cr	00	> ,, (1)	1	M 8. Cr not given by H, but confirmed by F
30·6					Bright streak
31·15		0000	36·37 (0)		*u*
31·58		000	28·55 (1)		*u*
32·0			36·37 (0)		*u*, ? double
32·68		000	28·55 (1)		
33·12	Fe	7	>69·72 (8)	8, winged	M 5, winged
34·02		000	36·37 (0)	00	*u*
34·26	V	0000	] 37·49 (1)		
34·38		000		00	
34·5					Bright streak
34·79	*p* Fe (F)	2	36·37 (0)	1	M – 3, thinned and winged
35·35	Co	000	,, (0)	00-0	M 4
35·67	Ni	00	37·49 (1)		On less refrangible side of Fe line ·56 (1)
36·37		0	,, (1)		
37·49	*p* Cr (F)	1	36·37 (0)	0-1	M – 3. R gives Cr ?

Wave-length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5238·01		0000	36·37 (0)		*u*
38·42		0000	,, (0)		*u*
38·5					Bright streak
38·74	Ti	000	34·79 (2)	1	M 7
39·14	Cr	00	39·99 (1)	0	M 6
39·99	Sc (L)	1			Thinned or unaffected
40·52		0000	39·99 (1)		
40·64	Cr (H)	000			Cr confirmed by F
41·04	V	000	36·37 (0)	00	M 2. Kayser gives Y at 40.96
41·62	Cr (H)	0000	< ,, (0)		Cr confirmed by F
42·24		000	39·99 (1)		*u*
42·66	Fe	2	50·82 (3)		M 6
43·53	Cr	00	43·95 (1)	0	
43·95	Fe	1			M 4. Widening not observed by F
44·70		000	36·37 (0)		*u*
45·12		0000	,, (0)		*u*, ? double
45·80		0000	,, (0)		*u*
45·90		0000			
46·31		0000	,, (0)		*u*. H gives Ti 46·30, but not found by F
46·73	Ti (H)	0000	39·99 (1)	00-0	Ti confirmed by F
47·23	Fe	1	47·74 (2)	1-2	M 4, narrow revl. once
47·74	Cr	2	63·49 (4)	2-3	M 5, revd. twice
48·09	Co	000	49·28 (00)		M 3
48·55		000	,, (00)		*u*
49·28	Fe	00	47·23 (1)	0	M 3, hazy
49·5			52·15 (0)		*u*
50·39	Fe	2	50·82 (3)	3	M 8, revd. three times
50·82	Fe	3	63·49 (4)		Darkened
51·7			52·15 (0)	·67 = 00-0	*u*
52·28	Ti	000	50·39 (2)	2 (+ Fe ·15)	Hangs on red side of Fe 52·15 (0). M 7
53·21		00	47·23 (1)	0	
53·42		0000			M 4
53·63	Fe	2	50·82 (3)		
54·13		0000	52·15 (0)		*u*
54·3					Bright streak
54·83	Co (H)	0000	52·15 (0)		
55·12	Fe, Cr (H)	3	63·49 (4)	4	
55·30	Cr	0	53·63 (2)		M 4
55·49	Mn	0		0-1	M 6
55·6					Bright streak
55·91		000	47·23 (1)	1	
55·97	Ti	0000			
56·8			57·10 (0)		
57·10	Sr ?	00	60·56 (0)		? Double
57·81	Co (H)	0	52·15 (0-1)		M 4, Ti ? Co and Ti confirmed by F
58·5			57·81 (0)		*u*
59·0			,, (0)		*u*
59·4					Bright streak
59·66		000	60·56 (0)		*u*

Wave-length	Origin	Int. in Sun	Int. in Spots (F)	Int. in Spots (H and A)	Remarks
5260·14	Ti (H)	000	52·15 (0-1)	0	M 5. Ti confirmed by F
60·56	Ca	0	53·63 (2)	1	M 5, revd. 3 times
60·84		0000	] 60·56 (0)		]*u*
60·95		0000			
61·67		0000	„ (0)		*u*
61·88	Ca, Cr	3	66·74 (6)		Winged
62·42	Ca	3	< „ (6)		Winged
63·06		00	75·45 (1)		
63·49	Fe	4	<66·74 (6)		Winged. May include Ti 63·67
64·33	Cr	4	] 69·72 (8)	] 9	
64·42	Ca	3			M 3, darkened
64·6					Bright streak
64·98		0	Oblitd.	00	
65·32	Cr	00	75·45 (1)		M 4
65·73	Ca	3	] 66·74 (6)		
65·89	Ni, Cr	2			
66·14	Ti	0	75·45 (1)	0-1	M 5
66·74	Fe	6	<69·72 (8)		
67·27		000	] 75·45 (1)		
67·45		00			
68·78	Co	00	„ (1)		
69·13		000	60·56 (0)		
69·72	Fe	8	(10)	9	E_2. M 5, winged
70·44	Ca	3	] 69·72 (8)		
70·56	Fe	4			E_1

o

Comparison of Visual and Photographic Spectra.

From the comparative data given in the catalogue, it might appear that considerably greater detail is revealed in the visual than in the photographic observations. This is only true to a limited extent, for many of the extra visual lines are clearly seen even in the reproduction of the photograph given by Hale and Adams. The apparent difference probably thus arises partly from incomplete tabulation of the photographic spectrum. There are, in fact, very few lines of relatively small intensity in my list which do not appear in the reproduced photograph in the region common to the two (b_4 to 5231). As regards the lines which are relatively strong in the Fraunhofer spectrum, the photograph closely represents what is observed visually, apart from the occasional benefit of greater resolution in the latter case, but the difficulty of recognising moderate widenings or darkenings is in no way diminished.

To take a few examples, the three titanium lines at 5173·9, 5193·1, and 5210·6, always seem to me to be intensified in the spot, both visually and in the photograph, but neither Hale nor Mitchell include the first two among the lines which they consider to be affected. In the case of the two lines 5195·1, 5195·6, Mitchell agrees with me in classing both as affected, while Hale only tabulates the former. Again, Hale agrees with me in including 5255·12 among the affected lines, while Mitchell does not give it. In still other cases, all three are agreed, as 5198·9. Generally speaking, the greater the original intensity of a line the less appears to be the chance of agreement among different observers, unless the line should be clearly winged or otherwise specially marked.

It will be noted that on the average I have assigned

somewhat greater intensities to the spot lines, especially the fainter ones, than those given by Hale and Adams; this may perhaps be accounted for by the fact that the spectrum of the sun for comparison in the photograph is of about the same intensity as the spectrum of the spot, while in the visual observations it is much greater.

In spite of the divergences which have been mentioned, however, there can be no doubt that the photographs already obtained by Prof. Hale exhibit most of the details that can be visually observed with an instrument such as that employed by me. It must be recognised, however, that different observers are just as likely to differ in tabulating the photographic records as the visual spectrum, and that tables drawn up from photographs do not necessarily represent all the facts. This is particularly the case in regard to some of the stronger lines, as to which Hale and Adams remark, in conformity with general experience, that "it is always difficult to be certain of a slight variation in intensity."

The Umbra Lines and Bright Streaks.

There are two features of the spot spectrum which suggest that the umbra lines and bright streaks may represent the structure of a banded or fluted spectrum.

First, Secchi observed that the general darkness of the spot was not uniform throughout the spectrum, and pointed out the resemblance to the banded spectra of red stars. He wrote [1] :—"Outre ces modifications caracteristiques qu' éprouvent les raies, on voit aussi varier l'intensité lumineuse des différentes parties du spectre, surtout dans le rouge dans le jaune et dans le vert. On voit se former des bandes sombres, principalement entre les points B et C et dans

[1] *Le Soleil*, vol. i., p. 290, 2nd edition, Paris, 1875.

le voisinage du point D . . . . Ces variations d'intensité sont faciles à constater, mais difficiles à évaluer car elles ne présentent rien de bien défini. Le phénomène est cependant incontestable, et il est indépendant de l'elargissement des raies noires, car l'espace compris entre 9 et 13 W (850 et 895 K)[1] demeure très-brillant, malgré la dilatation des raies du calcium et du fer. Notons en passant, comme une particularité bien remarquable, que ces bandes ont une très grande ressemblance avec celles qu'on observe dans le spectre des etoiles rouges."

Father Cortie[2] also includes among the characteristic features of spot spectra "a want of uniformity in blackness in various regions of the spectrum, with parts sometimes obscured." Cortie further remarks (p. 265) that "spot bands which are sometimes seen in the spectra of sun-spots would also, if they represent, as seems likely, the spectrum of a compound, witness to a great reduction of temperature in the materials constituting the spot." The two bands in the red near 6381, 6390, have in fact been described by Young, Cortie and others, as presenting the appearance of flutings sharpest on the red side, while Mitchell has resolved them into their components.

Again, the constituent lines of a band spectrum, starting from different sub-heads, frequently cluster together, leaving gaps free from lines which are very similar in appearance to those observed in spot spectra, and shown so clearly in the photographs taken by Hale and Adams. The general appearance may in fact be likened to the structure of a banded spectrum in which the principal heads are not very strongly marked when viewed with high dispersion.

[1] Wave-lengths, 6192 and 6105.

[2] *Astrophysical Journal*, vol. xx., p. 263, November 1904. See also *Monthly Notices*, vol. xlvii., p. 20, 1886.

On the assumption that we have to deal with a banded spectrum, it seemed possible that the substance producing it might be more easily identified by the gaps than by the component dark lines, and it was this which led me to determine the positions of the bright streaks which are given in the catalogue. Others, outside the region b—E, were also noted and their positions estimated. In accordance with the observations of Secchi, Young and Dunér, indicating that the dark components could be traced in the spectrum away from spots, it was soon noted that some of the bright streaks occupied spaces between nebulous faint lines tabulated by Rowland.[1] Subsequently, as a result of his observations, Mitchell was inclined to the opinion that most of the (dark) umbra lines are not present in the photospheric spectrum at all, but the photographic results of Hale and Adams, so far as they are yet stated in tabular form, have shown that the great majority are really coincident with faint solar lines. The natural inference is that the substance producing the umbra lines is thinly distributed over the entire photosphere.

The comparison with terrestrial spectra, however, has so far yielded only negative results. Some of the umbra lines, as might be expected in view of their great number, fall near to faint metallic lines (5179·29 and 5206·71, for example, nearly coincide with very faint lines in the arc spectrum of vanadium), but on the ordinary principles of spectrum analysis, such isolated coincidences cannot be considered significant. Seeing that titanium is so prominent in the spots, and that the flutings of this element (or more probably of its oxide) are characteristic of the third type stars, special attention has been given to the investigation of this substance. Photographs of the "arc

[1] *Monthly Notices*, vol. lxv., p. 516, March 1905.

flame" spectrum of titanium oxide have been measured in detail, but they show no consistent relation to the structure of the spot spectrum. A special search has also been made for the origin of the two conspicuous spot bands near 6381 and 6390, but without success. Most of the metallic flutings met with fade towards the red, while the spot bands in question fade to the violet, and those of cobalt, which resemble the spot bands very closely in appearance, do not agree in position.

It may be of course that the umbra lines belong to the line spectra of unrecognised elements, but there is nothing improbable in the idea that the faint Fraunhofer lines with which they appear to correspond may form part of a banded spectrum. The necessary conditions for the presence of flutings in the solar spectrum are in fact clearly shown to exist by the appearance in the Fraunhofer spectrum of the cyanogen fluting beginning at 3883, which was first indicated by Lockyer in 1878.

Now that Hale and Adams have succeeded in recording the necessary data in a form beyond suspicion, laboratory work leading to chemical identification seems almost the only thing likely to throw further light on the subject.

It is desirable, however, that the varying darkness of the spectra of spots in different regions, as described by Secchi and Cortie, should receive careful attention in future observations.

Thinned and Obliterated Lines.

Concurrently with the observations of spot spectra, the spectra of numerous metallic prominences and disturbed regions of the chromosphere have been examined in the hope of obtaining data which might be useful for comparative study. Special attention has been given to the distinction between "high-level" and "low-level" lines, for the reason that Young has not often made this distinc-

tion, and sufficiently good eclipse photographs of the less refrangible parts of the spectrum are not yet available.

As a first result of the comparison with spots, I pointed out in a paper on the great spot of February 1905,[1] that the high level lines were not among those intensified in the spot, while common lines were chiefly those of iron, chromium, and calcium, which appear as strong Fraunhofer lines. Mitchell finds this to be in complete agreement with his observations and gives several examples of high-level chromospheric lines, which, so far from being intensified in spots, are thinned or obliterated.

In a more recent paper [2] I have given further details as to the behaviour of the high-level lines, and have shown that 30 of them are enfeebled in spots. From Sir Norman Lockyer's work, 5 of these are found to be enhanced lines of iron, 5 enhanced titanium, and one enhanced scandium; my own experiments have indicated that 4 more are enhanced lines of iron, one an enhanced chromium line, and one an enhanced line of titanium.

For the sake of completeness, details relating to the lines in the region b—E are reproduced below.

Wave-length (R)	Origin	Inten. and Character in Chromos (F)	Int. in Sun (R)	SPOTS. (F)	Hale and Adams	Mitchell	Remarks
5169·22	*p* Fe	40 l	4	Nearly oblitd.	·07 ·22 weak on red edge	-4	(1)
5186·07	*p* Ti (F)	10 l-s	2	Reduced to 1	—	-3	—
5188·86	*p* Ti	15 l-s	2	Reduced	·86 ·02 hazy towards viol't	2	(2)
5197·74	*p* Fe (F)	25 l	2	Reduced to 0	Reduced to (0-1)	—	—
5226·71	*p* Ti	20 l-s	2	,, 1	—	Oblitd. once	—
5234·79	*p* Fe (F)	25 l	2	,, 0	Reduced to 1	-3	—
5237·49	*p* Cr (F)	10 l	1	,, 0	,, (0-1)	-3	—
5264·98	—	15 l	0	Obliterated	,, 00	—	—

[1] *Monthly Notices*, vol. lxv., p. 513, March 1905.

[2] *Monthly Notices* vol. lxvi. p. 361, April 1906.

Remarks. (1) A close double in sun, Fe5169·07(3) and pFe 5169·22(4). Notched on red side, as first described by Mitchell. (2) A close double in sun, pTi 5188·86(2) and Ca 5189·02(3). Notch on violet side observed with difficulty, probably on account of widening of Ca line.

The most definite conclusion is that the enhanced lines of iron, titanium, and chromium appear as high-level lines in the chromosphere, and that the corresponding Fraunhofer lines are enfeebled in the spectra of spots. It is probable that the vapours producing the enhanced lines are chiefly restricted to the higher chromospheric levels, but it is not yet clear whether the reduced intensities in spots is due to the withdrawal of these vapours from over the spot, or to the absence of a sufficiently luminous background to strongly exhibit their absorption.

As already remarked, the widened and weakened lines described by Mitchell are not clearly distinguished from the thinned and obliterated lines with such dispersion as I have been able to employ. Lines of this type show no distinct relation to the spectrum of the chromosphere *as hitherto recorded*, and the cause of their weakening in spots needs much further inquiry.

The Widened Lines.

The most obvious method of investigating the widened lines which occur simultaneously is to compare, element by element, the intensities of the lines in spots with those of the corresponding lines observed under different experimental conditions. It is impossible to believe that the widening is not systematic, and if the Fraunhofer lines corresponding to a given element are not proportionately affected, groups of lines which behave in a similar manner must evidently be sought.

The flame, arc, and spark provide three typical modes of producing spectra, and, in the case of some elements, the

relative intensities of the lines are different under these conditions. The flame spectrum, which may be most conveniently obtained by observing the "flame" of the electric arc, frequently only exhibits the brighter lines of the arc spectrum, but, in the case of iron especially, there are characteristic differences in the relative intensities of the lines in flame and arc. In the spark spectrum, the enhanced lines are the special feature, and though these have not yet been experimentally produced without admixture with arc lines, the fact that they exist almost alone in Alpha Cygni, and probably also at certain levels in the chromosphere, indicates that they form a distinct and important group. Three stages in the spectrum of the same element are thus in some cases available for comparison with the spot spectrum.

The existing records of spot spectra are inadequate for a complete discussion of the widened lines from this point of view. My own detailed observations extend over only a very small part of the spectrum, and the observations are too difficult to ensure certainty as to all the intensities assigned. The observation over a larger region made at the Solar Physics Observatory, South Kensington, and at Kodaikanal, as well as those made by Mitchell, and occasionally by myself, do not include all the lines that are certainly affected, as is clearly indicated by comparison with the photographic results of Hale and Adams, and with each other. The photographic records will doubtless yield better data for such a discussion, but comparison with the visual records suggests that all the affected lines are not included even in the extensive tables given by Hale and Adams. It is possible, of course, that the differences in the records are partly due to actual variations in the spot spectrum, but all have been made about the time of sun-spot maximum, and my own experience of the b—E

region during fifteen months has revealed no obvious changes.

No definite conclusions can accordingly be reached until there is a closer agreement among different observers as to the lines affected, and until the records are sufficiently complete to indicate the behaviour of every line of an element under investigation. Even then, all difficulties will not be removed. Our modes of stating intensities leave much to be desired, and there are very few elements for which all the lines tabulated can be accepted without question. A further difficulty is caused by the frequent occurrence of lines of compound origin, and the possibility that some lines are of composite origin although at present only attributed to one element. Another point, to which I have previously drawn attention, is that a general increase in the intensity of a spectrum produces a more marked effect on the faint than on the strong lines; Kirchhoff and Bunsen,[1] in fact found that "In general an indistinct line becomes brighter upon increasing the illumination, more rapidly than does a brighter line, but not to such an extent that the indistinct line ever overtakes in intensity the brighter one." [2] In other words, the fainter lines are more sensitive than the stronger ones to changes in the general intensity of the spectrum, and possibly some allowance for this difference may have to be made, especially when the lines are more darkened than widened.

Notwithstanding these difficulties, it may be useful to refer to the lines of some elements in order to emphasize

[1] *Phil. Mag.*, series 4, vol. xx., p. 94 (1860).

[2] A simple experiment which illustrates this may be made by photographing a spectrum when the slit is covered with a piece of thinly-smoked glass (or a portion of an old negative) having the middle part left clear. The central portion thus exhibits a brighter spectrum than the outer, and the fainter lines look "more widened" than the stronger ones.

the need for such detailed observations as I have attempted in the b—E region.

My previous observations in the red end suggested that, within the limits of error, the relative intensities of the lines of a given element in spots were the same as those of the corresponding lines in the Fraunhofer or arc spectrum, but Hale and Adams state that their results do not support this conclusion. Apart, however, from the special behaviour of the enhanced lines, to which reference has already been made, it may be premature to deny this general intensification in the case of some elements, notably calcium and chromium; but a more general discussion of the data now available certainly indicates that my previous suggestion is not directly applicable to all elements. Chromium and iron will provide sufficient illustrations of the detailed discussion which seems most likely to lead to definite conclusions.

Chromium. About half the lines of this element in the b—E region are coincident, or nearly so, with lines of other substances, and two lines given by Rowland at 5221·2 and 5222·5 are not confined by Hasselberg or myself. The facts with regard to the remainder are as follows: —

λ and Intensity in arc (Hasselberg)	λ and Intensity in Sun (Rowland)	Intensity in Spot (Hale and Adams)	Intensity in Spot (Fowler)	Conspicuousness in Spot (Mitchell)	(Kodaikanal)
5192·17 (3)	·16 (00)	0	1		
93·66 (1½)	·67 (000)	00 (u)	0		
5200·33 (1½)	·36 (00)	00-0	0	3	
14·30 (1½)	·29 (00)	0	0		
21·90 (2)	·93 (0)	1	1	4	7
24·22 (1)	·24 (000 N)	00-0	0		7
25·98 (1½)	·97 (000)		0	5	7
28·25 (1½)	·27 (000)		0		
39·13 (2)	·14 (00)	0	1	6	6
41·62 (1)	·62 (0000)		(00)		
43·53 (2)	·53 (00)	0	1		8
47·68 (6)	·74 (2)	2-3	4	5	
65·31 (3)	·32 (00)		1	4	

It will be seen that all but two of the lines have been recorded by more than one observer, and, according to my own observations, all the lines have occurred together. The relative intensities in the spots are certainly not materially different from those in the arc or solar spectrum, and are such as might result from a general strengthening of all the lines of this element.

To carry the inquiry a step further, I have made the following summary of the lines of chromium given by Hale and Adams (λ 5009 to λ 5853). These are sufficiently numerous for the partial elimination of error in the estimation of intensities by taking averages. Enhanced lines, lines of compound or doubtful origin, and those which fall too close to adjacent lines for clear separation in the spot spectrum have been omitted.

No. of lines	Intensity in Sun	Mean Intensity in Spots	Possible Anomalous Lines
5	000	00·0	
11	00	0	5387·8=1
10	0	1·0	5781·4=2
3	1	2·0	
4	2	2·9	
3	3	4·0	
3	4	6·0	
2	5	6·5	

The table shows very clearly that there is a general strengthening of all the lines included, the fainter ones being raised by one step in the scale of intensities and the stronger ones by a little more. Only two of the 41 lines in question can reasonably be regarded as anomalous, and as none of the figures can be considered absolute, a difference of one step from the mean may not be significant.

There are several lines attributed to chromium by Rowland which do not appear among the spot lines tabulated by Hale and Adams, but four of them are given by Mitchell, some appear in the Kodaikanal catalogue,[1] and

[1] *Bulletin*, No. iv.

others have been noted in my occasional observations outside the b—E region. In the case of chromium, therefore, the present data can scarcely be regarded as affording sufficient ground for the rejection of the simple supposition that all the lines are strengthened, at least approximately, in proportion to their ordinary arc intensities (or solar intensities if the enhanced lines be excluded).

In the case of chromium, practically all the arc lines appear also in the arc-flame spectrum, and there are no striking changes in their relative intensities. The behaviour of the chromium lines in spots accordingly does not very clearly distinguish between the two conditions, but there is nothing inconsistent with the supposition, suggested by a preliminary investigation of the iron lines, that the additional chromium absorption in spots may be produced by vapours at a temperature somewhat lower than that of the arc.

Iron. The study of the iron arc spectrum, either by Lockyer's valuable method of "longs and shorts," or by separately photographing the spectra of the arc and arc-flame, indicates important differences in the properties of the lines. Two lines at 5404·36 and 5405·99 furnish a good illustration; these are of about equal intensity in the arc and in the Fraunhofer spectrum, but the former is a short line and is relatively very weak in the flame, while the latter is a long line and remains as a strong line in the flame. Many other examples indicate, in accordance with the prior work of Lockyer, that the longest lines, which are characteristic of the flame, are not necessarily the strongest in the arc.[1] The following table will indicate the com-

[1] Another series of short lines is found if the spectrum be observed close to either of the poles; these are the enhanced lines and they do not appear in the flame; they are enfeebled in the spots, as already stated.

parative behaviour of a few typical lines over a considerable range of the sun-spot spectrum.

Iron lines which are strong in arc and flame				Iron lines which are strong in arc but much weaker in flame			
Wave-length	Int. in Sun	Spots H and A	Spots F	Wave-length	Int. in Sun	Spots H and A	Spots F
5227·36	5	—	7	5340·12	6	—	—
5233·12	7	8	9	5367·67	6	—	—
5269·72	8	9	10	5370·17	6	—	—
5328·24	8	—	10	5383·58	6	—	6-7
5397·34	7	9	9	5404·36	5	5-6	5-6
5405·99	6	8	8	5424·29	6	—	6-7
5429·91	6	8	8	5445·26	4	—	4-5
5434·74	5	—	6-7	5463·17	3	—	—
5447·13	6	8	8	5463·49	3		
5497·74	5	6	6-7	5476·50	1	—	—
5507·00	5	7	7	5476·78	3		

Of the iron lines which are weak in the arc but survive in the flame, 5225·70, 5247·23, and 5249·28 are notable examples, each of which is consistently noted as widened in spots. Examples of weak arc lines, which do not appear in the flame, or appear with reduced intensity, are 5180·23, 5235·56, and 5243·95; these do not appear to be intensified in the spots.

Although the matter needs much further investigation, it will be seen that the two groups of lines show a marked difference of behaviour in the spots, in spite of the general difficulty attending the observation of intensification in the case of strong Fraunhofer lines. The special intensification of the lines characteristic of the flame is at least sufficiently indicated to encourage a more searching examination of all the lines of iron from this point of view.

Other elements. It is undesirable to multiply details as to other elements until more complete data relating to the spots are available, but a few general remarks may be made.

A comparison of the lines of *vanadium* observed in spots

with those recorded in the arc spectrum brings out the fact that it is only the strongest lines of the arc spectrum which appear at all in spots. In the region 5657·67 to 5747·98 inclusive, Hasselberg tabulates 20 lines due to this element, and only the eleven lines of intensity greater than five in the arc occur in spots; the observations by Mitchell, Hale and Adams, and myself, and those made at Kodaikanal, are in absolute agreement as to these lines. They are practically the only lines which survive in the arc-flame spectrum in this region, and the absence of the fainter arc lines from spots suggests that the vanadium vapour approximates in temperature to that of the flame. The low intensity in the spots of the lines of vanadium at 5234·26 and 5241·04 also furnishes evidence in the same direction, as these lines are relatively much weaker in the flame than in the arc. It should be noted, however, that the line at 5176·95 appears with too great an intensity in the spots to agree with its intensity either in the arc or flame, but it occurs in a region where umbra lines are numerous, and vanadium may not be the sole origin.

Summarising the intensities of the lines of *titanium* in the same manner as those of chromium (p. 224), it is found that out of 44 isolated lines given by Hale and Adams, there are six which may be considered as unduly strengthened in the spots as compared with the solar intensities, while the intensification of the fainter lines generally is in excess of that found for chromium. The titanium lines seem to be more darkened than widened, and it is possibly this fact which gives greater prominence to the fainter lines. As to the "anomalous" lines, the flame spectrum gives no assistance, and, apart from the possibility of coincident lines of unrecognised elements, no explanation is apparent. It may be, of course, that the conditions under which the vapours exist in spots are such as to

modify the intensities of some of the lines, but so far we have no experimental results to guide us.

A comparison of the lines recorded in spots with the arc spectrum of *scandium* tabulated by Lockyer and Baxandall,[1] indicates that the scandium lines which appear in the spots are among the strongest in the arc spectrum, while the possible coincidences with the fainter arc lines are so few as to be probably accidental. At the same time there are some strong arc lines which are not intensified in the spots; it is a noticeable fact that these are more intense in the solar spectrum than those which appear in the spots, and the probability is that all of them, like 5527·03, are related to the enhanced line class. The flame spectrum of this element has not yet been recorded.

General conclusion. The general result of the preliminary discussion is to suggest that while the enhanced lines of some elements are usually reduced in intensity in the spot spectrum, the arc lines are intensified in accordance with their intensities in the arc-flame, thus suggesting that the additional absorption is produced by relatively cool vapours. This result was in fact long ago arrived at by Sir Norman Lockyer, who stated [2] that "many of the lines seen in spots are lines seen at low temperatures (some of them in the oxyhydrogen flame) and none of them are those brightened or intensified when we pass from the temperature of the electric arc to that of the electric spark." Such a reduction of temperature also accords well with the presence of spot bands, as already remarked by Cortie and others.

It is not claimed that this view will remove every difficulty met with in attempts to interpret the spot

[1] *Roy. Soc. Proc.*, vol. lxxiv., p. 538, 1905.
[2] *Chemistry of the Sun*, p. 314, 1887.

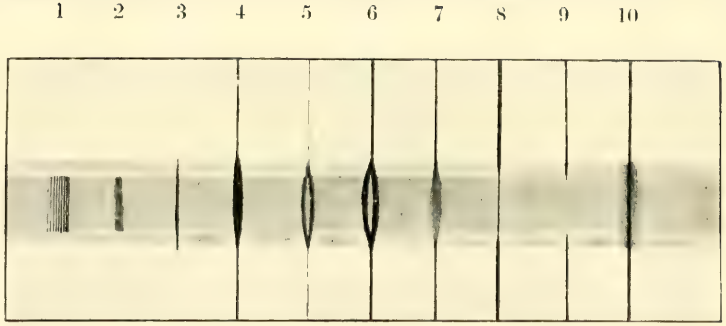


FIG. 1.—TYPES OF SUN-SPOT LINES (MITCHELL).

1. Spot band, as at 6390. 2. Umbra line. 3, 4. Types of widened lines. 5, 6. Widened lines with centres reversed bright. 7. Widened and weakened line. 8. Thinned and weakened line. 9. Obliterated line. 10. Winged line.

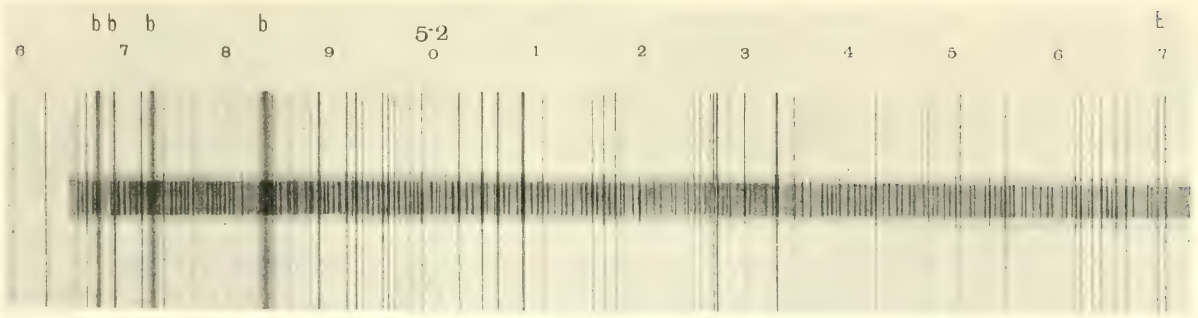


FIG. 2. MAP GIVING DETAILS OF SUN-SPOT SPECTRA, REGION b—E.

spectrum, but simply that it demands full investigation before we call in the aid of extraordinary actions to account for the phenomena observed. Much more extensive observations of spot spectra and more laboratory investigations are needed before any final conclusions can be drawn.

Royal College of Science,
London,
April, 1906.

Description of Plate.

Fig. 1 represents the different types of lines observed in spot spectra, as drawn by Mitchell, with the addition of a 'spot band.'

Fig. 2 illustrates the author's map of the spot spectrum in the region b—E, drawn on a copy of Higgs's photograph of this part of the spectrum. The map is necessarily to some extent diagrammatical, and the reproduction is not quite perfect. In the original the details were inserted in pencil and Chinese white, and some difficulty has been experienced in making a satisfactory block.

PART VIII.

Resolutions passed at the Oxford Conference and Text of the Constitution as finally adopted.

No. 29*a*.

Resolutions concerning Standards of Wave-length.

1. The wave-length of a suitable spectroscopic line shall be taken as the primary standard of wave-length. The number which defines the wave-length of this line shall be fixed permanently and thereby define the unit in which all wave-lengths are to be measured. This unit shall differ as little as possible from 10^{-10} metres and be called the Ångström.

2. Secondary standards are required at distances not greater than 50 Ångström units. These secondary standards should be referred to the primary standard by means of an interferometer method. The source of light should be obtained by means of an electric arc of from 6 to 10 Ampères.

3. A Committee shall be appointed to select the standards and to organise the determination of their wave-lengths in terms of the primary standard in at least two independent laboratories.

4. The same Committee shall be charged with the selection of tertiary standards which shall be at distances of from 5 to 10 Å. The wave-lengths of these tertiary standards are to be obtained by interpolation with the help of gratings.

No. 30*a*.

Resolutions concerning the measurement of Solar Radiation.

1. In order to secure uniformity, it is desirable that observations on the intensity of solar radiation in different localities shall be made as far as possible with the same type of instrument.

2. That for the present Ångström's pyrheliometer be adopted as the standard instrument.

3. That it is desirable to obtain accurate comparisons between the records of Ångström's pyrheliometer and other standard instruments; and that Mr. Abbot, Professor Callendar, Professor Wladimir Michelson, and Mr. W. E. Wilson be asked to assist the Union in this work.

4. That for the determination of the possible changes in the solar-radiation power, it is desirable to secure measurements of the intensity over limited ranges of the spectrum, which are not affected by absorption due to ozone, aqueous vapour, and carbonic acid.

5. That a Committee be appointed to draw up a scheme of co-operation, and proposals for the reduction of observations. That the Committee be requested to communicate the scheme and proposals to the Executive Committee, with a view of initiating a system of observations according to the scheme.

6. That the meeting recognises the great importance of measurements by direct photography, as well as by other methods, of the relative intensities of

radiation emitted by different parts of the solar surface, and desires to include such measurements in the subjects to be dealt with by the Union.

No. 31*a*.

Resolutions concerning co-operation in the investigation of the Sun's surface.

1. Co-operation is desirable in the various branches of Solar Research, such as visual and photographic observations of the solar surface, visual observations of prominences, and observations of the solar atmosphere with spectroheliographs of various types.
2. When an institution has collected and co-ordinated results from various sources, members of the Union shall be requested to place their observations at the disposal of the said institution.
3. In the case of investigations which have not yet been thus collected and co-ordinated, special committees nominated by the Union shall be charged with the work of preparation and organisation. That the Committees be requested to communicate the schemes and proposals to the Executive Committee with a view of initiating a system of observations according to the scheme.
4. It is proposed forthwith to organise, through the Union, such co-operation in two branches of research:
   (*a*) The study of the spectra of sun-spots.
   (*b*) The study of the records, by means of the H and K light, of phenomena of the solar atmosphere.

5. The Union lays special stress on the fact that, notwithstanding the obvious utility of co-operation in certain cases, individual initiative is the chief factor in a very large number. It is as much the duty of the Union to encourage original researches as to promote co-operation.

No. 32*a*.

Resolutions concerning the relations of the Union to the International Association of Academies.

That the Union request the International Association of Academies to allow itself to be placed at the head of the list of bodies constituting the Union.

That the International Association of Academies be asked to agree to nominate one of the three members of the Executive Committee. The Union suggest that the nomination of this member may conveniently be made by the leading Academy, the nomination holding, while that Academy continues to act as leading Academy.

No. 33*a*.

Resolution concerning the desirability of forming additional Committees for co-operation with the Union.

That in the opinion of the Conference, it would be desirable that countries which do not contain scientific bodies forming part of the Union should organise solar research by forming Committees which could co-operate with the Union.

No. 34*a*.

RESOLUTION CONCERNING THE INTERPRETATION OF RESOLUTIONS PRINTED IN DIFFERENT LANGUAGES.

That where a resolution is printed in different languages, the language of the country in which the meeting at which the resolution was passed was held, be taken as the authoritative text.

No. 29*b*.

RÉSOLUTIONS CONCERNANT LES ETALONS DE LONGUEURS D'ONDE.

1. La longueur d'onde d'une radiation convenablement choisie sera prise comme étalon primaire des longueurs d'onde. Le nombre qui représente cette longuer d'onde sera fixé une fois pour toutes; il définira dès lors l'unité de longuer d'onde qui devra différer aussi peu que possible de 10^{-10} mètre et s'appellera "Ångström."
2. Il y a lieu de choisir des étalons secondaires dont la distance ne dépassera pas 50 Ångström. Ces étalons secondaires seront rapportés à l'étalon primaire par une méthode interférentielle. La source lumineuse sera fournie par un arc électrique de 6 à 10 Ampères.
3. Un comité sera nommé pour choisir les étalons et organiser les déterminations de ces longueurs d'onde relativement à l'étalon primaire au moins dans deux laboratoires indépendants.
4. Le même Comité sera chargé de choisir des étalons tertiaires placés à des distances variant de 5 à 10 Ångström. Les longueurs d'onde de ces étalons tertiaires seront obtenues par interpolation avec des réseaux.

No. 30*b*.

Résolutions concernant la mesure de la Radiation Solaire.

1. Pour assurer l'uniformité des observations il est à désirer que les observations de l'intensité de la radiation solaire soient faites dans les différentes localités autant que possible avec le même type d'instrument.

2. Actuellement le pyrhéliomètre d'Ångström sera adopté comme instrument étalon.

3. Il est à désirer que des comparaisons précises soient faites entre l'instrument d'Ångström et les autres instruments étalons, et que M. Abbot, le Professeur Callendar, le Professeur Wladimir Michelson, et M. W. E. Wilson, soient priés d'assister l'Union pour ce travail.

4. Pour déterminer des changements possibles dans l'intensité de la radiation solaire, il est désirable d'assurer l'exécution des mesures de l'intensité dans des régions du spectre déterminées, où les effets dus à l'ozone, à la vapeur d'eau et à l'acide carbonique ne se font pas sentir.

5. Il y a lieu de nommer un Comité chargé de rédiger un plan de coopération et de faire des propositions pour la rédaction des observations. Ce comité est prié de communiquer le plan et les propositions au Comité exécutif pour l'organisation du système des observations suivant le plan tracé.

6. La réunion reconnait la grande importance des mesures faites par la photographie directe aussi bien que par d'autres méthodes, pour étudier

comparativement l'intensité de la radiation émise par les différentes parties de la surface du soliel, et souhaite comprendre de semblables mesures dans les sujets rentrant dans la sphère d'action de l'Union.

No. 31*b*.

Résolutions concernant les recherches sur les phenomènes de la surface du Soleil.

1. La coopération des observateurs est souhaitable dans les diverses recherches relatives au soleil, telles que les observations visuelles et photographiques de la surface, les observations visuelles des protubérances et les observations de l'atmosphère solaire faites à l'aide des spectrohéliographes de types divers.
2. Lorsqu'une institution aura déjà réuni et coordonné des résultats d'études faites par divers observateurs, les membres de l'Union seront invités à mettre leurs observations à la disposition de cette institution.
3. Relativement aux recherches dont les résultats ne sont pas encore réunis et coordonnés, l'Union nommera un comité spécial chargé de préparer et d'organiser les recherches. Ce comité devra communiquer son plan et ses propositions au Comité exécutif qui organisera un systéme d'observations en conformité du plan transmis.
4. Une semblable coopération peut être organisée dès maintenant par l'Union dans deux branches de recherches :

   1°—L'étude du spectre des taches;

   2°—L'étude avec enregistrement des phénomènes de l'atmosphère solaire par l'observation des radiations H et K.

5. L'Union insiste sur ce fait que, si la coopération présente une utilité évidente pour certaines recherches, l'initiative individuelle reste, dans un grand nombre d'autres cas, le facteur principal. Il est autant du devoir de l'Union d'encourager les recherches originales que de provoquer la coopération.

No. 32*b*.

RÉSOLUTIONS CONCERNANT LES RELATIONS DE L'UNION AVEC L'ASSOCIATION INTERNATIONALE DES ACADEMIES.

L'Union prie l'Association Internationale des Académies de se mettre à la tête des corporations scientifiques qui composent l'Union.

L'Association Internationale des Académies est priée de nommer un des trois membres de la Commission Exécutive. L'Union suggère que cette nomination pourrait être faite convenablement par l'Académie directrice et que la personne ainsi désignée devrait conserver ses fonctions jusqu'au changement de l'Académie directrice.

No. 33*b*.

RÉSOLUTION CONCERNANT LA DÉSIRABILITÉ DE FORMER DES COMMISSIONS SPÉCIALES DANS LES PAYS QUI N'ONT PAS DE SOCIÉTÉS S'OCCUPANT DE RECHERCHES SOLAIRES.

Dans l'opinion de la conférence il est à désirer qu'on forme des commissions spéciales pour conférer avec l'Union, dans les pays qui n'ont pas de corporations scientifiques faisant partie de l'Union.

No. 34*b*.

RÉSOLUTION CONCERNANT L'INTERPRETATION DES RESOLUTIONS IMPRIMÉES DANS PLUSIEURS LANGUES.

Si une résolution est imprimée en plusieurs langues, la version officielle est celle du pays où a eu lieu la conférence qui à approuvé la résolution.

No. 29*c*.

BESCHLÜSSE ÜBER DIE FESTSTELLUNG DER WELLENLÄNGEN NORMALEN.

1. Die Wellenlänge einer geeigneten Spectrallinie soll als Hauptnormale der Wellenlänge angenommen werden. Die Wellenlänge dieser Linie soll ein für alle Mal festgesetzt werden und damit die Einheit angeben, nach welcher alle übrigen Wellenlängen zu messen sind. Diese Einheit soll möglichst wenig von 10^{-10}m. abweichen, und sie soll Ångström heissen.
2. Normalen zweiter Klasse sollen gewählt werden, welche nicht mehr als je 50 Ångström von einander entfernt liegen. Sie sollen mit der Hauptnormale durch Interferenzmethoden verknüpft werden. Als Lichtquelle soll der Bogen dienen, der mit einer Stromstärke von 5 bis 10 Amp. brennt.
3. Es soll ein Ausschuss ernannt werden, welcher die Aufgabe hat, die Normalen zu wählen und die Messung ihrer Wellenlängen in mindestens zwei unabhängigen Laboratorien in die Wege zu leiten.
4. Dieser Ausschuss wird weiter beauftragt, Normalen dritter Klasse festzusetzen, welche in Abständen von je 5 bis 10 Ångström liegen sollen. Ihre Wellenlängen werden durch Interpolation in Gitterspectren ermittelt.

No. 30*c*.

BESCHLÜSSE WELCHE DIE MESSUNG DER SONNENSTRAHLUNG BETREFFEN.

1. Um vergleichbare Resultate zu erzielen, ist es wünschenswerth, dass die Messungen über Inten-

sität der Sonnenstrahlung an verschiedenen Orten so viel wie möglich mit Instrumenten gleicher Art ausgeführt werden.

2. Ångströms Pyrheliometer soll zunächst als Normalinstrument genommen werden.

3. Es ist wünschenswerth, die Angaben von Ångströms Pyrheliometer mit denen anderer Instrumente möglichst genau zu vergleichen. Die Herren Abbot, Prof. Callendar, Prof. Wladimir Michelson und W. E. Wilson, sollen gebeten werden, die Vereinigung bei dieser Arbeit zu unterstützen.

3. Zur Erkennung eventueller Veränderlichkeit in der Stärke der Sonnenstrahlung ist es wünschenswerth, Messungen der Intensität in beschränkten Spectralgebieten auszuführen, in welchen sich keine Absorption durch Ozon, Wasserdampf oder Kohlensäure geltend macht.

5. Es soll ein Ausschuss ernannt werden, welcher Pläne für die gemeinsamen Arbeiten und Vorschläge für die Reduction der Beobachtungen ausarbeitet. Der Ausschuss soll gebeten werden, diese Pläne und Vorschläge dem Arbeitsausschuss der Vereinigung mitzutheilen, damit die entsprechenden Beobachtungen in die Wege geleitet werden.

6. Die Versammlung erkennt es als sehr wichtig an, dass durch directe Photographie oder andere Methoden die relative Intensität der Strahlung verschiedener Theile der Sonnenoberfläche gemessen wird, und wünscht solche Messungen in ihr Arbeitsprogramm aufzunehmen.

No. 31*c*.

Beschlüsse welche sich auf die Untersuchungen der Erscheinungen auf der Sonnenoberfläche beziehen.

1. Ein Zusammenarbeiten ist auf verschiedenen Gebieten der Sonnenforschung wünschenswerth,

z.B. bei der Beobachtung der Sonnenoberfläche mit Auge und Photographie, bei der Beobachtung von Protuberanzen mit dem Auge, bei der Beobachtung der Sonnenatmosphäre mit verschiedenen Formen des Spectroheliographen.

2. Wenn ein Institut Resultate aus verschiedenen Quellen bereits gesammelt und bearbeitet hat, so sollen die Mitglieder der Vereinigung ersucht werden, ihre Beobachtungen demselben zur Verfügung zu stellen.

3. Für Beobachtungen, welche noch nicht in dieser Art gesammelt und bearbeitet worden sind, sollen besondere Ausschüsse von der Vereinigung ernannt werden, welche die Beobachtungen zu sammeln und deren Bearbeitung zu organisiren haben. Die Ausschüsse sollen ersucht werden ihre Pläne und Vorschläge dem Arbeitsausschuss mitzutheilen, damit die entsprechenden Beobachtungen in die Wege geleitet werden.

4. Es wird vorgeschlagen, dass die Vereinigung gleich das Zusammenarbeiten für zwei Gebiete der Forschung organisirt:

   (*a*) die Untersuchung von Spectren der Sonnenflecken.

   (*b*) die Untersuchung der Erscheinungen der Sonnenatmosphäre, wie sie durch das Licht der H- und K-Linie angezeigt werden.

5. Es ist besonders zu betonen, dass trotz des offenbaren Nutzens eines Zusammenarbeitens in manchen Fällen doch in sehr vielen anderen individuelle Anregung der wichtigste Factor ist. Die Vereinigung betrachtet es daher ebenso für ihre Aufgabe, individuelle Forschungen zu unterstützen, wie ein Zusammenarbeiten zu fördern.

No. 32*c*.

Beschlüsse über die Beziehungen der Vereinigung zur Internationalen Association der Akademieen.

Die Vereinigung ersucht die Internationale Association der Akademieen sich an die Spitze der wissenschaftlichen Körperschaften zu stellen, welche sich zur gemeinsamen Erforschung der Sonne vereinigt haben.

Die Internationale Association der Akademieen wird gebeten eines der drei Mitglieder des Arbeitsausschusses zu ernennen. Die Vereinigung schlägt vor, dass dieses Mitglied von derjenigen Akademie ernannt wird, welche den Vorort bildet, und im Amt bleibt, bis eine Aenderung im Vorort stattfindet.

No. 33*c*.

Beschluss über die Zweckmässigkeit weiterer Ausschüsse zur Mitwirkung mit der Vereinigung.

Die Conferenz hält es für wünschenswerth, dass in denjenigen Ländern, in denen keine Körperschaften bestehen, welche der Vereinigung angehören, die Sonnenforschung eingeleitet werde durch besondere Ausschüsse, welche mit der Vereinigung zusammen wirken können.

No. 34*c*.

Beschluss über die Ausdeutung der in verschiedenen Sprachen abgefassten Bechlüsse.

Falls ein Beschluss in verschiedenen Sprachen gedruckt ist, soll die Sprache des Landes als Hentschëidend betrachtet werden in welcher die Conferenz stattgefunden hat, auf welcher der Beschluss gefasst wurde.

No. 35*a*.

Constitution of the International Union for Co-operation in Solar Research.

STATUTES.

I.—Constitution of the Union.

1. The Union shall consist of Scientific Bodies interested in the progress of Solar Research. Each constituent body shall act through a Committee nominated for the purpose. Each member of such Committee shall be considered a member of the Union. There may also be corresponding members who shall be members of the Union.

2. An International Committee shall be organised through the election of one member by each constituent body.

3. There shall be an Executive Committee consisting of three members of the International Committee. Two of the three shall be elected by the International Committee at one of its meetings, and shall hold office until the following meeting.[1]

4. Additions to the number of bodies constituting the Union may be made by the International Committee, provided that at least two constituent bodies unite in recom-

[1] As indicated by the resolution No. 32, the International Association of Academies has been asked to nominate the third member.

mending such additional body. The corresponding members shall be selected by the International Committee.

II.—Meetings of the Union.

5. There shall, as a rule, be biennial assemblies of the Union for the purpose of discussing

(*a*) Questions of administration and organisation.

(*b*) Scientific questions relating to Solar Physics.

In case the Executive Committee is of opinion that there is not sufficient matter of interest for discussion, they may communicate with the constituent bodies, who shall then decide by a vote, taken by correspondence, whether the biennial meeting shall be postponed to another year, provided that no such postponement shall take place unless a majority of two-thirds is obtained.

The meetings for the discussion of questions of administration and organisation shall be confined to members of the International Committee, and shall be referred to as meetings of the International Committee.

Scientific questions shall be discussed at meetings in which all members of the Union may take part; such meetings shall be referred to as meetings of the Union.

The assembly of the Union shall consist partly of meetings of the Union, and, if necessary, partly of meetings of the International Committee.

Each constituent body may send as many delegates to the meetings of the Union as wish to take part in its proceedings, but each body shall only have one vote, which shall be cast (unless the society specially nominates another of its members) by the representative of the society on the International Committee.

6. The Executive Committee may invite any person interested in questions of Solar Physics, but not belonging to any constituent body, to take part in the meetings of the Union. Such invited guests shall have no vote, but shall have the right to take part in all scientific discussions.

III.—Procedure for Discussion of Scientific Questions.

7. Any of the constituent bodies of the Union, or any member of the Union, may send a proposal to the Executive Committee to include a particular question of Solar Physics among the problems to be dealt with by the Union. If the proposal comes from a society, and if the question is included among the general subjects of investigation approved by the International Committee, the Executive Committee shall act as provided for in Articles 9 and 10. If the proposal comes from a member of the Union, the Executive Committee shall have power to act or not according to its discretion.

8. A subject may also be included amongst those to be dealt with, by a resolution of a meeting of the Union.

9. When a subject is included amongst those to be dealt with by the Union, the Executive Committee shall inform each of the constituent societies of the fact and ask it to express an opinion on the subject, either collectively or through any of its members. The Executive Committee may also ask the advice of any worker specially interested in the subject, though he may not be a member of the Union.

10. The Executive Committee may then make further additions, and shall circulate the completed report among the constituent societies.

11. The Executive Committee shall have power to receive reports which may be drawn up by any of the constituent bodies or by any member of the Union: but such reports, not being invited by the Executive Committee, shall only be published by a resolution of the Union at its next meeting.

12. No important resolution shall be adopted as a resolution of the Union except through a vote of the Union or of the International Committee at one of their meetings. But this regulation shall not be held to prevent the Union from the deputing of specific powers of decision to a Committee appointed at a meeting of the Union.

13. Nothing in the above shall be interpreted as interfering with the full liberty of any society or any one of its members to act independently or to prosecute any research on their own behalf.

IV.—Limitation of Subject.

14. The subjects to be dealt with by the Union shall refer to:

(1) Organisation of observations and their methods of reduction.

(2) The conduct of laboratory experiments bearing on solar problems.

The Union as such shall not undertake the discussion of the relative merits of rival theories, though it may organise observations or experiments likely to throw light on such theories..

V.—Place of Meeting.

15. At each of the meetings of the International Committee, the place of the subsequent meeting shall be determined, and the chairman who is to preside at that meeting shall be elected. The date of the meeting shall either be

fixed at the same time or left to the decision of the Executive Committee, who shall in that case consult, by correspondence, the members of the International Committee.

VI.—Officers of the Union.

16. There shall be a Chairman of the Executive Committee elected by the International Committee, who shall be responsible for the interchange of correspondence in the intervals between the meetings, and for the publication of any matter the printing of which shall be decided upon either by the International Committee or by the Executive Committee.

In the case of correspondence carried on between constituent bodies on matters connected with the work of the Union, it is desirable that copies of such correspondence, if affecting the work of the Union, shall be communicated to the Chairman of the Executive Committee.

17. There shall be a Central Bureau (not necessarily of a permanent character) charged with the conduct of the correspondence and publications of the Union, and of such other duties as may be deputed to it by the Union. The Central Bureau shall be under the direction of the Chairman of the Executive Committee.

18. There shall be a Computing Bureau under the direction of one of the members of the International Committee. The work of the Computing Bureau shall be the conduct of such matters concerning calculations and reductions of observations as may be decided upon by the Executive Committee. The International or Executive Committee may, however, charge any other institution or member of the Union with the discussion and reduction of observations dealing with special portions of the subject. The Director of the Computing Bureau shall have power to

initiate and carry out in the Bureau any calculations or reductions connected with the work of the Union, provided that the work decided upon by the Executive Committee shall not thereby be delayed.

VII.—Amendments.

19. The above regulations may be added to or amended by a vote of the Union at one of its meetings provided that notice of the proposed amendment or addition has been given at least three months before the meeting.

No. 35*b*.

Constitution de l'Union Internationale pour la Coopération dans les Recherches sur le Soleil.

STATUTS.

I.—Constitution de l'Union.

1. L'Union se composera de Sociétés scientifiques s'intéressant au progrès des recherches sur le soleil. Chaque société faisant partie de l'Union sera représentée par une commission élue à cet effet. Les membres de ces Commissions seront considérés comme membres de l'Union.

Il y aura des membres correpondants qui seront également membres de l'Union.

2. Un Comité international sera formé; chacune des Sociétés constituant l'Union désignera un de ses membres pour en faire partie.

3. Une Commission exécutive composée de trois membres du Comité international sera instituée; deux de ces membres seront élus par le Comité international à l'une de

ses réunions, et resteront en fonctions jusqu' à la réunion suivante.[1]

4. Le Comité International pourra augmenter le nombre des Sociétés constituant l'Union à condition toutefois que toute société nouvelle soit patronnée par au moins deux des sociétés constituant l'Union. Les membres correspondants seront élus par le Comité international.

II.—Assemblées de l'Union.

5. L'Union s'assemblera tous les deux ans pour la discussion :

(*a*) des questions d'organisation et d'administration;
(*b*) des questions d'ordre scientifique se rapportant à la Physique Solaire.

Si la Commission exécutive estime que les sujets à discuter ne sont pas assez nombreux, elle pourra en faire part aux sociétés constituant l'Union, celles-ci décideront, par un vote par correspondance, si l'assemblée biennale doit être remise à une autre année ou non; toutefois, pour que la remise soit prononcée, la majorité des deux tiers des voix est nécessaire.

Les membres du Comité international assisteront seuls aux réunions consacrées à la discussion des questions d'organisation et d'administration, ces réunions constitueront les Assemblées du Comité international.

Les questions d'ordre scientifique seront discutées dans des réunions auxquelles tout membre de l'Union pourra participer; ce seront les Assemblées de l'Union.

Le Congrès de l'Union se composera partie d'Assemblées de l'Union, partie d'Assemblées du Comité international, quand ce sera nécessaire.

[1] Comme indiqué par la résolution No. 32 l'Association Internationale des Académies est priée de nommer le troisième membre de la commission executive.

Chaque société constituante pourra envoyer aux assemblées de l'Union, comme délégués, ceux de ses membres qui désireront y prendre part, mais chaque société ne disposera que d'une voix attribuée au représentant de la Société au Comité international (sauf le cas où la Société désignera expressement un autre de ses membres).

6. La Commission exécutive pourra inviter les personnes s'intéressant aux questions de la Physique solaire, n'appartenant pas à une des Sociétés constituantes, à assister aux Assemblées de l'Union. Ces invités n'auront pas voix délibérative, mais auront le droit de prendre part à toutes les discussions scientifiques.

III.—Procedure a suivre pour la Discussion des Questions Scientifiques.

7. Toute société constituante ou tout membre de l'Union pourra proposer à la Commission exécutive d'inscrire une question particulière parmi les problèmes à traiter par l'Union. Quand la proposition sera faite par une Société, et si la question est comprise parmi les sujets généraux de recherches, approuvés par le Comité international, la Commission exécutive appliquera les articles 9 et 10. Quand la proposition sera faite par un membre de l'Union, la Commission exécutive sera autorisée à agir par elle-même.

8. Un sujet peut également être inscrit parmi les sujets à traiter à la suite d'une délibération de l'Assemblée de l'Union.

9. Lorsqu'un sujet aura été admis au nombre de ceux que l'Union doit traiter, la Commission exécutive en informera chacune des Sociétés constituantes et leur demandera de formuler leur opinion sur ce sujet, soit sous une forme collective, soit par l'intermédiaire d'un de ses membres.

La Commission exécutive pourra aussi demander l'avis de personnes s'intéressant spécialement à ce sujet, sans qu'elles soient membres de l'Union.

10. La Commission exécutive pourra alors faire encore des additions et mettra le rapport complété en circulation parmi les Sociétés constituantes.

11. La Commission exécutive sera autorisée à recevoir des rapports rédigés par n'importe quelle société constituante ou n'importe quel membre de l'Union, mais ces rapports, n'ayant pas été demandés par la Commission exécutive, ne seront publiés que sur avis favorable donné par l'Union à l'assemblée suivante.

12. Aucune résolution importante ne peut être acceptée comme résolution de l'Union si elle n'a été l'objet d'un vote favorable de l'Union ou du Comité international à l'une de leurs séances. Cette règle ne privera cependant pas l'Union du droit de déléguer le pouvoir de prendre des résolutions à une Commission nommée par l'une de ses assemblées.

13. Rien de ce qui précède ne doit être interprêté comme susceptible de gêner la pleine liberté d'une société ou d'un membre d'une société, qui pourront agir indépendamment et poursuivre des recherches pour leur propre compte.

IV.—Limitation des Sujets a Traiter.

14. Les sujets que l'Union doit traiter se rapporteront à :

(*a*) l'organisation d'observations et celle des méthodes de réduction.

(*b*) la direction d'expériences touchant à des problèmes solaires.

L'Union n'entreprendra pas la discussion des mérites relatifs de théories rivales, quoiqu'elle puisse instituer des observations ou des expériences susceptibles d'apporter quelque lumière dans les discussions de ces théories.

V.—Siege des Assemblees.

15. Lors de chaque séance du Comité international on déterminera l'endroit où aura lieu la séance suivante, on élira en même temps le futur président de l'Assemblée, on fixera la date de la réunion; celle-ci en conférera par correspondance avec le Comité international.

VI.—Des Bureaux.

16. Le Président de la Commission exécutive sera élu par le Comité international, il sera responsable de l'échange de la correspondance dans les intervalles des assemblées, et des publications décidées soit par le Comité international, soit par la Commission exécutive.

En ce qui regarde la correspondance entre deux sociétés constituantes, dont le sujet se rapporte aux travaux de l'Union, il est désirable que la copie de cette correspondance soit communiquée au président de la Commission exécutive, si elle touche aux travaux de l'Union.

17. Un bureau central formé à cet effet sera chargé de la direction de la correspondance et des publications de l'Union ainsi que de toute autre mission dont l'Union pourra le charger, Le bureau central (qui ne sera pas nécessairement permanent) sera placé sous le contrôle du président de la Commission exécutive.

18. Un bureau de calcul sera constitué sous la direction d'un membre du Comité international. Ce bureau s'occupera selon les décisions de la Commission exécutive de ce qui aura trait aux calculs et à la rédaction des observations.

La Commission exécutive et le Comité international peuvent cependant charger n'importe quelle société ou un membre particulier de l'Union de la discussion et de la rédaction d'observations se rapportant à des questions d'un

sujet spécial. Le Directeur du Bureau de calcul sera autorisé à prendre l'initiative de calculs et de réductions concernant le travail de l'Union et à le faire exécuter par le Bureau de calcul, pourvu que le travail décidé par la Commission exécutive ne soit pas retardé de ce fait.

VII.—Modifications aux Statuts.

19. Des additions ou des modifications peuvent être faites aux réglements ci-dessus par un vote de l'Assemblée de l'Union, à condition que ces modifications ou additions soient proposées au moins 3 mois à l'avance.

No. 35*c*.

Constitution der internationalen Vereinigung für gemeinsame Sonnenforschung.

STATUTEN.

I.—Zusammensetzung der Vereinigung.

1. Die Vereinigung soll aus wissenschaftlichen Körperschaften bestehen, welche sich für den Fortschritt der Sonnenforschung interessiren. Jede dieser Körperschaften soll sich durch einen zu diesem Zwecke ernannten Ausschuss an den Verhandlungen der Vereinigung betheiligen. Jedes Mitglied eines solchen Ausschusses gilt als Mitglied der Vereinigung. Daneben können auch correspondirende Mitglieder zugelassen werden. Jeder Ausschuss soll eines seiner Mitglieder wählen; diese bilden zusammen einen internationalen Ausschuss.

3. Es soll ein Arbeitsausschuss aus drei Mitgliedern der internationalen Ausschusses gebildet werden; zwei von ihnen werden von dem internationalen Ausschuss in einer

Versammlung gewählt, und bleiben bis zur nächsten Versammlung im Amte.[1]

4. Auf Vorschlag wenigstens zweier der Vereinigung angehörender Körperschaften können weitere Körperschaften durch den internationalen Ausschuss in die Vereinigung aufgenommen werden. Correspondirende Mitglieder werden durch den internationalen Ausschuss gewählt.

II.—Versammlungen der Vereinigung.

5. Es soll jedes zweite Jahr eine Versammlung der Vereinigung statt finden zur Besprechung von

(*a*) Fragen der Verwaltung und Organisation,

(*b*) Wissenschaftlichen Fragen der Sonnenforschung.

Falls der Arbeitsausschuss meint, dass nicht genügender Stoff zur Verhandlung vorliegt, soll er dies den beigetretenen Körperschaften mittheilen; diese entscheiden dann durch schriftliche Abstimmung, ob die Versammlung um ein Jahr verschoben werden soll; doch ist dazu $\frac{2}{3}$ Mehrheit erforderlich.

Die Versammlungen zur Besprechung von Fragen der Verwaltung und Organisation sollen auf die Mitglieder der internationalen Ausschusses beschränkt bleiben, und sie sollen Sitzungen des internationalen Ausschusses heissen.

Wissenschaftliche Fragen werden in Versammlung en verhandelt, an welchen alle Mitglieder der Vereinigung Theil nehmen: sie sollen Sitzungen der Vereinigung heissen.

Bei einer Versammlung der Vereinigung werden also theils Sitzungen der Vereinigung, theils, wenn nöthig,

[1] Wie durch den Beschluss No. 32 angezeigt ist, wird die Internationale Association der Akademicen gebeten das dritte Mitglied des Arbeitsausschusses zu wählen.

Sitzungen des internationalen Ausschusses statt finden.

Jede theilnehmende Körpeschaft kann beliebig viele Delegirte zur Theilnahme an den Verhandlungen senden; aber jede Körperschaft soll bei den Abstimmungen nur eine Stimme haben, welche von dem Vertreter der Körperschaft bei dem internationalen Ausschuss abgegeben wird, falls nicht die Körperschaft ein anderes Mitglied besonders dafür erwählt.

6. Der Arbeitsausschuss kann Personen, welche zu keiner theilnehmenden Körperschaft gehören, sich aber für Fragen der Sonnenphysik interessiren, zu den Versammlungen der Vereinigung einladen. Solche eingeladene Gäste können sich an allen wissenschaftlichen Verhandlungen betheiligen, aber nicht an den Abstimmungen.

III.—Die Vernandlungen über wissenschaftliche Fragen.

7. Jede Körperschaft, welche zur Vereinigung gehört, oder jedes einzelne Mitglied derselben, kann dem Arbeitsauschuss bestimmte Fragen aus der Sonnenphysik vorschlagen, damit diese von der Vereinigung behandelt werden. Wenn der Vorschlag von einer Körpeschaft ausgeht und sich auf eine Frage bezieht, welche vom internationalen Auschuss als in den Kreis der Verhandlungen gehörig anerkannt ist, so soll der Arbeitsausschuss den Vorschlag entsprechend den § § 9 und 10 behandeln. Kommt aber der Vorschlag von einer einzelnen Person, so soll der Arbeitsausschuss das Recht haben, über seine Zulassung zu entscheiden.

8. Auch durch Beschluss einer Sitzung der Vereinigung kann eine Frage in den Kreis der Verhandlungen eibezogen werden.

9. Wenn eine Frage zur Besprechung durch die Vereinigung angenommen worden ist, so soll der Arbeitsausschuss

jede theilnehmende Körperschaft davon benachrichtigen und sie ersuchen, sich dazu durch ein Gutachten entweder ihres Ausschusses oder eines Mitgliedes desselben zu äussern. Der Arbeitsausschuss kann auch die Meinung eines Forschers, der sich besonders für diese Frage interessirt, einziehen, auch wenn er nicht Mitglied der Vereinigung ist.

10. Der Arbeitsausschuss soll dann aus den Gutachten und weiteren Bemerkungen einen vollständigen Bericht verfassen, der den theilnehmenden Körperschaften zugesandt wird.

11. Der Arbeitsausschuss soll das Recht haben, von jeder theilnehmenden Körperschaft oder von jedem Mitgliede der Vereinigung Berichte entgegen zu nehmen; aber wenn solche Berichte von dem Arbeitsausschuss nicht erbeten waren, so werden sie nur veröffentlicht, falls dies bei der nächsten Versammlung beschlossen wird.

12. Kein wichtiger Beschluss soll als Beschluss der Vereinigung gelten, wenn er nicht durch eine Abstimmung der Vereinigung oder des internationalen Ausschusses in einer Sitzung angenommen worden ist. Aber die Vereinigung behält sich vor, besondere Rechte der Beschlussfassung an Ausschüsse zu übertragen, welche in einer Sitzung der Vereinigung ernannt sind.

13. Keine der vorstehenden Regeln soll die Bedeutung haben, als wolle sie die volle Freiheit einer Körperschaft oder ihrer Mitglieder beschränken, nach eigenem Ermessen zu handeln oder Untersuchungen fortzuführen.

IV.—Aufgaben der Vereinigung.

14. Die Aufgaben der Vereinigung sollen sich beziehen auf:

1. Die Organisation von Beobachtungen und die Art ihren Verwerthung.

2. Die Ausführung von Versuchen im Laboratorium, welche mit Problemen der Sonne zusammenhängen.

Die Vereinigung soll als solche nicht die Besprechung des relativen Werthes verschiedener Theorien unternehmen; aber sie kann Beobachtungen oder Versuche veranlassen, welche zu einer Entscheidung über solche Theorien führen.

V.—Versammlungsort.

15. Bei jeder Versammlung des internationalen Ausschusses wird der Ort für die nächste Versammlung und der Vorsitzende für dieselbe bestimmt. Das Datum der Versammlung soll entweder gleichzeitig festgestellt werden, oder seine Festsetzung wird dem Arbeitsausschuss überlassen, welcher dazu schriftlich die Meinung der Mitglieder des internationalen Ausschusses einholt.

VI.—Beamte der Vereinigung.

16. Der internationale Ausschuss ernennt einen Vorsitzenden des Arbeitsausschusses. Derselbe hat die Correspondenz in der Zeit zwischen den Versammlungen zu vermitteln und die Veröff entlichung der Schriftstücke zu überwachen, deren Druck von dem internationalen oder dem Arbeits-Ausschuss beschlossen worden ist.

Falls zwischen den theilnehmender Körperschaften eine Correspondenz über Fragen stattfindet, welche das Arbeitsgebiet der Vereinigung betreffen, so ist es wünschenswerth, dass Abschriften davon dem Vorsitzenden des Arbeitsausschusses mitgetheilt werden.

17. Es soll ein Centralbureau eingerichtet werden, wenn auch nicht auf die Dauer. Dasselbe steht unter Leitung des Vorsitzenden des Arbeitsausschusses; es soll die Corre-

spondenz führen, die Veröffentlichungen oder andere Aufträge, welche ihm von der Vereinigung übertragen werden, besorgen.

18. Es soll ein Rechenbureau eingerichtet werden unter der Direction eines Mitglieder des internationalen Ausschusses. Seine Aufgabe ist die Berechnung oder Reduction von Beobachtungen, welche ihm vom Arbeitsausschuss überwiesen werden. Der internationale oder Arbeitsausschuss kann indessen auch irgend ein anderes Institut oder Mitglied der Vereinigung mit der Discussion oder Berechnung von Beobachtungen betrauen, welche irgend einen speciellen Theil der Frage betreffen. Der Director des Rechenbureaus soll berechtigt sein, auch andere Rechnungen und Reductionen, welche sich auf das Arbeitsgebiet der Vereinigung beziehen, zu veranlassen und durchzuführen, falls dadurch die von dem Arbeitsausschuss gegebene Arbeit nicht verzögert wird.

VII.—Aenderung der Bestimmungen.

19. Die vorstehenden Bestimmungen können ergänzt oder verbessert werden durch eine Abstimmung in einer Sitzung der Vereinigung, falls der Antrag dazu wenigstens drei Monate vorher eingereicht worden ist.

MANCHESTER UNIVERSITY PUBLICATIONS.

Medical Series. No. 1.

No. I. SKETCHES OF THE LIVES AND WORK OF THE HONORARY MEDICAL STAFF OF THE ROYAL INFIRMARY. From its foundation in 1752 to 1830, when it became the Royal Infirmary. By Edward Mansfield Brockbank, M.D., M.R.C.P. Crown 4to (illustrated). 15s. net.

"Dr. Brockbank's is a book of varied interest. It also deserves a welcome as one of the earliest of the 'Publications of the University of Manchester.'"—*Manchester Guardian.*

"We have a valuable contribution to local Medical Literature."—*Daily Dispatch.*

Medical Series. No. 2.

No. II. PRACTICAL PRESCRIBING AND DISPENSING. For Medical Students. By William Kirkby, sometime Lecturer in Pharmacognosy in the Owens College, Manchester. Crown 8vo, 220 pp. Second edition. 5s. net.

"The whole of the matter bears the impress of that technical skill and thoroughness with which Mr. Kirkby's name must invariably be associated, and the book must be welcomed as one of the most useful recent additions to the working library of prescribers and dispensers."—*Pharmaceutical Journal.*

"Thoroughly practical text-books on the subject are so rare that we welcome with pleasure Mr. William Kirkby's 'Practical Prescribing and Dispensing.' The book is written by a pharmacist expressly for medical students, and the author has been most happy in conceiving its scope and arrangement."—*British Medical Journal.*

"The work appears to be peculiarly free from blemishes and particularly full in practical detail. It is manifestly the work of one who is a skilled chemist, and an expert pharmacist, and who knows not only the requirements of the modern student but the best way in which his needs may be met."—*Medical Press.*

"This is a very sensible and useful manual."—*The Hospital.*

"The book will be found very useful to any students during a course of practical dispensing."—*St. Bartholomew's Hospital Journal.*

"The book is a model, being tutorial from beginning to end."—*The Chemist and Druggist.*

"A very useful little book."—*Practitioner.*

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

HISTORICAL SERIES. No. 1.

No. III. MEDIÆVAL MANCHESTER AND THE BEGINNING OF LANCASHIRE. By JAMES TAIT, M.A., Professor of Ancient and Mediæval History. Demy 8vo, 240 pp. 7s. 6d. net.

"Patient and enlightened scholarship and a sense of style and proportion have enabled the writer to produce a work at once solid and readable."—*English Historical Review.*

"A welcome addition to the literature of English local history, not merely because it adds much to our knowledge of Manchester and Lancashire, but also because it displays a scientific method of treatment which is rare in this field of study in England."—Dr. Gross in *American Historical Review.*

"La collection ne pouvait débuter plus significativement et plus heureusement que par un ouvrage d'histoire du Moyen Age dû a M. Tait, car l'enseignement mediéviste est un de ceux qui font le plus d'honneur à la jeune Université de Manchester, et c'est a M. le Professeur Tait qu'il faut attribuer une bonne part de ce succès."—*Revue de Synthèse historique.*

"The two essays are models of their kind."—*Manchester Guardian.*

ECONOMIC SERIES. No. 1.

No. IV. THE LANCASHIRE COTTON INDUSTRY. By S. J. CHAPMAN, M.A., M. Com. Jevons Professor of Political Economy and Dean of the Faculty of Commerce. 7s. 6d. net. Demy 8vo.

"Such a book as this ought to be, and will be, read far beyond the bounds of the trade."—*Manchester Guardian.*

"There have been books dealing with various phases of the subject, but no other has so ably breathed it from the economic as well as from the historical point of view."—*Manchester Courier.*

"The story of the evolution of the industry from small and insignificant beginnings up to its present imposing proportions and lightly developed and specialised forms, is told in a way to rivet the attention of the reader . . . . . the book is a valuable and instructive treatise on a fascinating yet important subject."—*Cotton Factory Times.*

"Highly valuable to all close students."—*Scotsman.*

HISTORICAL SERIES. No. 2.

No. V. INITIA OPERUM LATINORUM QUAE SAECULIS XIII., XIV., XV. ATTRIBUUNTUR. By A. G. LITTLE, M.A., Lecturer in Palæography. Demy 8vo, 300 pp. (interleaved). 15s. net.

MEDICAL SERIES. No. 3.

No. VI. HANDBOOK OF SURGICAL ANATOMY. By G. A. WRIGHT, B.A., M.B. (Oxon.), F.R.C.S., and C. H. PRESTON, M.D., F.R.C.S., L.D.S. Crown 8vo, 214 pp. Second edition. 5s. net.

"We can heartily recommend the volume to students, and especially to those preparing for a final examination in surgery."—*Hospital.*

"Dr. Wright and Dr. Preston have produced a concise and very readable little handbook of surgical applied anatomy. . . . The subject matter of the book is well arranged and the marginal notes in bold type facilitate reference to any desired point."—*Lancet.*

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

HISTORICAL SERIES. No. 3.

No. VII. THE OLD COLONIAL SYSTEM. By GERALD BERKELEY HERTZ, M.A., B.C.L. Demy 8vo, 232 pp. 5s. net.

"Mr. Hertz gives us an elaborate historical study of the old colonial system, which disappeared with the American Revolution . . . . He shows a remarkable knowledge of contemporary literature, and his book may claim to be a true history of popular opinion."—*Spectator.*

"Mr. Hertz's book is one which no student of imperial developments can neglect. It is lucid, fair, thorough, and convincing."—*Glasgow Herald.*

"Mr. Hertz's 'Old Colonial System' is based on a careful study of contemporary documents, with the result that several points of no small importance are put in a new light . . . . it is careful, honest work . . . . The story which he tells has its lesson for us."—*The Times.*

"Both the ordinary reader and the academic mind will get benefit from this well-informed and well-written book."—*Scotsman.*

ECONOMIC SERIES. No. 2. (GARTSIDE REPORT, No. 1.)

No. VIII. AN EXAMINATION OF THE COTTON INDUSTRY IN THE UNITED STATES. By T. W. UTTLEY, B.A. Demy 8vo. 1s. net.

"Mr. Uttley is to be congratulated on the performance of a not altogether easy task, and his book, in conception and execution, appears to fulfil admirably the intentions of the Trust."—*Manchester Courier.*

"The writer gives ample details concerning wages and other features connected with typical mills . . . and the information thus gathered is of interest and value to the factory operative as well as the student and economist."—*Cotton Factory Times.*

"Mr. Uttley describes how he visited the mills in various States in a very systematic and detailed manner. Altogether the report makes an admirable and welcome collection of information, and will be found on many occasions worthy of reference."—*Textile Mercury.*

THEOLOGICAL SERIES. No. 1.

No. IX. INAUGURAL LECTURES delivered during the Session 1904–5, by the Professors and Lecturers of the Faculty of Theology, viz. :—

Prof. T. F. Tout, M.A.; Prof. A. S. Peake, B.D.; Prof. H. W. Hogg, M.A.; Prof. T. W. Rhys Davids, LL.D.; Rev. W. F. Adeney, D.D.; Rev. A. Gordon, M.A.; Rev. L. Hassé; Rev. Canon E. L. Hicks, M.A.; Rev. H. D. Lockett, M.A.; Rev. R. Mackintosh, D.D.; Rev. J. T. Marshall, D.D.; Rev. J. H. Moulton, D.Litt.

Edited by A. S. PEAKE, B.D., Dean of the Faculty.

Demy 8vo, 300 pages. 7s. 6d. net.

"The lectures, while scholarly, are at the same time popular, and will be found interesting and instructive by those who are not theologians.

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

PRESS NOTICES (Inaugural Lectures)—*Continued.*

. . . The entire series is excellent, and the volume deserves a wide circulation."—*Scotsman.*

"This is a very welcome volume . . . All these lectures were delivered to popular audiences, yet they are far from superficial, and will be found of great value to busy pastors and teachers."—*Christian World.*

"We welcome the volume as a most auspicious sign of the times."—*Spectator.*

"The lectures themselves give a valuable conspectus of the present position of Theological research . . . They are, of course, not addressed to experts, but they are exceedingly valuable, even when allowance is made for their more or less popular form."—*Examiner.*

"The whole volume forms a very important and valuable contribution to the cause of Theological learning."—*Record.*

"This is a most interesting and valuable book, the appearance of which at the present moment is singularly significant. . . . But it is impossible in a brief review to indicate all the treasures of this rich volume, to read which carefully is to be introduced to the varied wealth of modern Biblical scholarship."—*Baptist.*

"This volume is of the most exceptional value and interest."—*Expository Times.*

"This is a book of more than common interest."—*Review of Theology and Philosophy.*

"The writers of these lectures do not attempt to offer more than samples of their wares : but what is given is good, and it may be seen that theology without tests is destitute neither of scientific value nor of human interest."—*Athenæum.*

ANATOMICAL SERIES. No. 1.

No. X. STUDIES IN ANATOMY from the Anatomical Department of the University of Manchester. Vol. iii. Edited by ALFRED H. YOUNG, M.B. (Edin.), F.R.C.S., Professor of Anatomy. Demy 8vo, 320 pp., 24 Plates. 10s. net.

MEDICAL SERIES. No. 4.

No. XI. A COURSE OF INSTRUCTION IN OPERATIVE SURGERY in the University of Manchester. By WILLIAM THORBURN, M.D., B.S. (Lond.), F.R.C.S., Lecturer in Operative Surgery. Crown 8vo. 2s. 6d. net.

"This little book gives the junior student all that he wants, and nothing that he does not want. Its size is handy, and altogether for its purpose it is excellent."—*University Review.*

"As a working guide it is excellent."—*Edinburgh Medical Journal.*

PUBLIC HEALTH SERIES. No. 1.

No. XII. ARCHIVES OF THE PUBLIC HEALTH LABORATORY OF THE UNIVERSITY OF MANCHESTER. Edited by A. SHERIDAN DELÉPINE, M.Sc., M.B., Ch.M. Crown 4to, 450 pp. £1. 1s. net.

"The University of Manchester has taken the important and highly commendable step of commencing the publication of the archives of its

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

Public Health Laboratory, and has issued, under the able and judicious editorship of Professor Sheridan Delépine, the first volume of a series that promises to be of no small interest and value alike to members of the medical profession and to those of the laity . . . Original communications bearing upon diseases which are prevalent in the districts surrounding Manchester, or dealing with food- and water-supplies, air, disposal of refuse, sterilisation and disinfection and kindred subjects, will be published in future volumes; and it is manifest that these, as they successively appear, will form a constantly increasing body of trustworthy information upon subjects which are not only of the highest interest to the profession but of supreme importance to the public."—*The Lancet.*

"It is safe to say that as these volumes accumulate they will form one of the most important works of reference on questions of public health, and ought, at all events, to be in the library of every public authority."—*Manchester Guardian.*

"The volume . . . . speaks well for the activity of investigation in Manchester."—*Lancet.*

PHYSICAL SERIES. No. 1.

No. XIII. THE PHYSICAL LABORATORIES OF THE UNIVERSITY OF MANCHESTER. A record of 25 years' work. Demy 8vo, 160 pp, 10 Plates, 4 Plans. 5s. net.

This volume contains an illustrated description of the Physical, Electrical Engineering, and Electro-Chemistry Laboratories of the Manchester University, also a complete Biographical and Bibliographical Record of those who have worked in the Physics Department of the University during the past 25 years.

MEDICAL SERIES. No. 5.

No. XIV. A HANDBOOK OF LEGAL MEDICINE. By W. SELLERS, M.D. (London), of the Middle Temple and Northern Circuit, Barrister-at-law. With Illustrations. Crown 8vo. 7s. 6d. net.

"This is quite one of the best books of the kind we have come across."—*Law Times.*

MEDICAL SERIES. No. 6.

No. XV. A CATALOGUE OF THE PATHOLOGICAL MUSEUM OF THE UNIVERSITY OF MANCHESTER. Edited by J. LORRAIN SMITH, M.A., M.D. (Edin.), Professor of Pathology. Crown 4to, 1260 pp. 7s. 6d. net.

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

Historical Series. No. 4.

No. XVI. STUDIES OF ROMAN IMPERIALISM. By W. T. Arnold, M.A. Edited by Edward Fiddes, M.A., with Memoir of the Author by Mrs. Humphrey Ward and C. E. Montague. With a Photograuve of W. T. Arnold. Demy 8vo, 400 pp. 7s. 6d. net.

"Mrs. Humphry Ward has used all her delicate and subtle art to draw a picture of her beloved brother; and his friend Mr. Montague's account of his middle life is also remarkable for its literary excellence."—*Athenæum.*

"The memoir . . . . . tenderly and skilfully written by the 'sister and friend,' tells a story, which well deserved to be told, of a life rich in aspirations, interests, and friendships, and not without its measure of actual achievement."—*Tribune.*

"Readers of this fragment will note the writer's generally facile style, his large grasp in detail, his preference for the study of historical matters as illustrative of the progress of communities rather than as records of individual character and achievement; and will understand the loss to literature of a mind capable of such zealous, honourable, and determined effort."—*Globe.*

"This geographical sense and his feeling for politics give colour to all he wrote."—*Times.*

"The singularly interesting literary monument which in the introduction to this volume has been raised to the memory of the late William Arnold by the affection of his distinguished sister, and by the whole-hearted comradeship of a fellow-worker of many years, will be welcome to the numerous friends whom he has left behind him."—*Spectator.*

"Anyone who desires a general account of the Empire under Augustus which is freshly and clearly written and based on wide reading will find it here."—*Manchester Guardian.*

"Nothing could be better than the sympathetic tribute which Mrs. Humphry Ward pays to her brother, or the analysis of his work and method by his colleague Mr. Montague. The two together have more stuff in them than many big books of recent biography."—

——— *Westminster Gazette.*

No. XVII. CALENDAR OF THE VICTORIA UNIVERSITY OF MANCHESTER. Session 1904–5. Demy 8vo, 1100 pp. 3s. net.

No. XVIII. CALENDAR OF THE VICTORIA UNIVERSITY OF MANCHESTER. Session 1905–6. Demy 8vo, 1100 pp. 3s. net.

No. XIX. CALENDAR OF THE VICTORIA UNIVERSITY OF MANCHESTER. Session 1906–7. Demy 8vo, 1100 pp. 3s. net.

Medical Series. No. 7.

No. XX. HANDBOOK OF DISEASES OF THE HEART. By Graham Steell, M.D., F.R.C.P., Lecturer in Diseases of the Heart, and Physician to the Manchester Royal Infirmary. Crown 8vo. 400 pp., 11 plates (5 in colours), and 100 illustrations in the text. 7s. 6d. net.

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

Economic Series, No. 3. (Gartside Report, No. 2.)

SOME MODERN CONDITIONS AND RECENT DEVELOPMENTS IN IRON AND STEEL PRODUCTION IN AMERICA, being a Report to the Gartside Electors, on the results of a Tour in the U.S.A. By Frank Popplewell, B.Sc. Demy 8vo. [*In the press.*

Educational Series. No. 1.

CONTINUATION SCHOOLS IN ENGLAND AND ELSEWHERE: Their place in the Educational System of an Industrial and Commercial State. By Michael E. Sadler, M.A., LL.D., Professor of the History and Administration of Education. Demy 8vo.

This work is largely based on an enquiry made by past and present Students of the Educational Department of the University of Manchester. Chapters on Continuation Schools in the German Empire, Switzerland, Denmark, and France, have been contributed by other writers. [*In the press.*

Historical Series.

CANON PETER CASOLAS' PILGRIMAGE TO JERUSALEM IN THE YEAR 1494. By M. Newett. Demy 8vo. [*In the press.*

Economic Series, No. 4. (Gartside Report, No. 3.)

ENGINEERING AND INDUSTRIAL CONDITIONS IN THE UNITED STATES. By Frank Foster, M.Sc. Demy 8vo. [*In the press.*

The following are in preparation and will be issued shortly :—

DISEASES OF THE EAR. By W. Milligan, M.D., Lecturer on Diseases of the Ear and Nasal Surgeon to the Manchester Royal Infirmary.

DISEASES OF THE EYE. By C. E. Glascott, M.D., Lecturer on Ophthalmology, and A. Hill Griffith, M.D., Ophthalmic Surgeon to the Manchester Royal Infirmary.

HANDBOOK OF NERVOUS DISEASES. By Judson S. Bury, M.D., Lecturer on Clinical Neurology and Physician to the Manchester Royal Infirmary.

MANCHESTER UNIVERSITY PUBLICATIONS—*continued.*

LECTURES.

GARDEN CITIES (Warburton Lecture). By Ralph Neville, K.C. 6d. net.

THE BANK OF ENGLAND AND THE STATE (A Lecture). By Felix Schuster. 6d. net.

BEARING AND IMPORTANCE OF COMMERCIAL TREATIES IN THE TWENTIETH CENTURY. By Sir Thomas Barclay. 6d. net.

THE SCIENCE OF LANGUAGE AND THE STUDY OF THE GREEK TESTAMENT (A Lecture). By James Hope Moulton, M.A., Litt.D. 6d. net.

Issued from the University Press.

MELANDRA CASTLE, being the Report of the Manchester and District Branch of the Classical Association for 1905. Edited by R. S. Conway, Litt.D. Introduction by Rev. E. L. Hicks, M.A. Demy 8vo. Illustrated. 5s. net.

"Both the antiquarian and the student of general English history should find this volume of deep interest."—*Tribune.*

"A thoroughly creditable piece of work, carefully edited. . . . Meanwhile we wish the excavators continued success, and hope that their spirited example will be followed in other parts of the country by competent classical scholars."—Mr. G. F. Hill in the *Manchester Guardian.*

"Many admirable plans and illustrations add to the clearness of this excellently planned book on a very interesting subject."—*Daily News.*

Prof. Willamowitz Moellendorf, of Berlin, writes of the Keltic and Roman Weights :—"This is really a great result and one on which the Corpus Inscriptionum is silent."

RECORD OF THE JUBILEE CELEBRATIONS AT OWENS COLLEGE, MANCHESTER. Crown 4to, 200 pp. (illustrated). 2s. 6d. net.

THE BOOK OF RUTH (Unpointed Text). 6d. net.

SCENES FROM THE RUDENS OF PLAUTUS, with a Translation into English Verse. Edited by R. S. Conway, Litt.D., Professor of Latin in the University. 6d. net.

Sherratt & Hughes
Manchester: 34 Cross Street
London: 60 Chandos Street W.C.

113361 P
Author International Union for Co-operation Aston
[in Solar Research I
Title Transactions. vol.1.

University of Toronto
Library

DO NOT
REMOVE
THE
CARD
FROM
THIS
POCKET

STORAGE

Acme Library Card Pocket
Under Pat. "Ref. Index File"
Made by LIBRARY BUREAU